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AN
ELEMENTARY SYSTEM
OF
PHYSIOLOGY.

By JOHN BOSTOCK, M. D. P. G. S.

F. R. S. L. S. AND H. S. M. R. I.

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VOL. II.

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**C. Baldwin, Printer,
New Bridge-street, London,**

TO
ARTHUR AIKIN, ESQ. F.L.S. & G.S.,
SECRETARY TO THE SOCIETY OF ARTS, &c.

MY DEAR SIR,

THE first volume of this Treatise I had proposed to have inscribed to our much valued friend, Dr Marcet, under whose inspection the work was commenced, and from whom I received many important suggestions, respecting both the plan and the mode of execution. His premature death prevented me from fulfilling my design. To the present volume I beg leave to prefix your name. A friendship, which may be said to have been transmitted to us from our parents, which commenced in our childhood, and which has continued to the present period, without the slightest interruption, might alone be sufficient to justify my choice. But, independently of any private feelings of this description, I am anxious to embrace

this opportunity of giving my public testimony to your excellent moral qualities, and to your varied scientific acquirements; qualities which are the more esteemed the more they are known; and, acquirements which have been uniformly employed in the improvement of the useful arts, or in the advancement of knowledge.

Believe me, my dear Sir,

Your very sincere and faithful Friend,

J. BOSTOCK.

Upper Bedford-Place,
March 5th, 1826.

PREFACE.

THE reception which the first volume of this treatise experienced, could leave no doubt in my mind as to the necessity of completing the work ; and I lament that certain circumstances, which were unavoidable, have delayed for so long a period the publication of the second part. As, however, the circumstances to which I allude no longer exist, I may indulge a hope that the third volume will succeed the present at a considerably shorter interval. I have, as nearly as possible, pursued the same method of giving an account of the best established facts, and the most approved hypotheses ; freely offering my remarks upon them, and pointing out any part which appeared to be objectionable. I have, however, found it a very difficult task to arrive at any satisfactory conclusion, with respect to many of the topics that are discussed in this volume. For, singular as it may appear, although most of them profess to be established on the basis of direct experiments, and such as would appear not to be of very difficult execution, yet we shall find that every step of the track through which I have had to pass, is on debateable ground. On this account, I have frequently felt it necessary to dissent from the opinions of the most eminent physiologists of the age ; but when I have done this,

I have given my reason for the dissent ; and I trust that I have, in no instance, gone beyond that candid criticism, which it is necessary to exercise on all scientific topics.

I beg to repeat the request with which I conclude the preface to my former volume, that my readers will use towards me the same liberty which I have used towards others ; that they will, without reserve, point out all the errors and imperfections which may be found in the work. I have thought it, upon the whole, more advisable to defer the notice of these remarks until the publication of my third and concluding volume ; as by this means I shall have the advantage of another year's experience, an advantage which is of no small moment in a science so rapidly progressive as that of Physiology.

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ELEMENTS

OF

PHYSIOLOGY.

CHAP. VII.

OF RESPIRATION.

NEXT to the circulation of the blood, the function which is the most essential to life, at least in the higher orders of animals, is respiration. Respiration consists in the alternate reception and emission of air into and out of the lungs, at the same time that the blood is transmitted through a set of vessels so situated, as to enable the air to act upon it, and to produce that change in its nature and properties, which fits it for the support of life.¹ I shall arrange

¹ It is scarcely necessary to remark, that the above description applies only to the higher orders of animals, the mammalia, birds, and amphibia. In fishes, the process which is equivalent to respiration is performed by the branchiæ or gills, which are placed in a passage communicating with the fauces, and terminating on the surface of the body, through which a portion of the water received into the mouth is forcibly propelled. It is thus brought into close approximation with the blood which circulates through their fringed extremities, where it receives

my remarks upon this subject under three heads; first, the mechanism of respiration; second, its direct effects; and third, its remote effects on the living system.

its appropriate change from the air which the water retains in solution. The mode in which the respiration of fishes is effected, which was imperfectly understood by Boyle, Works, v. i. p. 109, was correctly described by Mayow, Tract. i. c. 15 p. 259; although like many of his discoveries, it appears to have been forgotten, when it was again pointed out by Priestley; On Air, v. iii. p. 342, v. v. p. 136. et seq., of the 1st sec. and v. iii. p. 382 of the 2d series. The respiration of fishes has been since examined by Carradori; Ann. de Chim. t. xxix. p. 171, 2; and by Cuvier; Leçons d'Anatomie Comp. t. iv. p. 305, 6. We have also a very interesting and elaborate set of experiments on this subject by Humboldt and Provençal; Mem. d'Arcueil, t. ii. p. 359. et seq., to which I shall have occasion to refer more particularly in a subsequent section of this chapter. We have also some very valuable experiments by Dr. Edwards, particularly on the relation which the respiration of fishes bears to that of animals that are furnished with lungs; De l'Influence des Agens, &c. par. ii. ch. 3. M. Dumeril has given an elaborate dissertation on the mechanism of the respiratory organs of fishes; Nicholson's Journ. v. xxviii. p. 350. et seq. translated from Mag. Encyc. Nov. 1807, p. 35. In many of the invertebrated animals the respiratory organs consist merely of a number of tubes or pores, called tracheæ, provided with open mouths, which simply admit the air to enter into them, while there are numerous tribes in which no distinct apparatus can be detected: it appears, however, that in all cases the animal produces the same kind of change upon the air. Scheele noticed the effect of a leach in abstracting the oxygen from water; On Air and Fire, p. 167. Vauquelin found that insects and snails consume oxygen and generate carbonic acid; Ann. de Chim. t. xii. p. 273. et seq. Spallanzani repeated and diversified Vauque-

§ 1. *Mechanism of Respiration.*

The principal organs of respiration in man are the trachea with its ramifications, the pulmonary system of blood-vessels, the lungs, and the diaphragm. The first constitutes the passage by which the air is conveyed into its appropriate receptacles; the sanguiferous vessels are the apparatus by which the blood is carried through the lungs in such a manner, as to enable it to receive the influence of the air; these two sets of parts, with the connecting membranous matter, compose the lungs, while the diaphragm is the principal agent in the alternate enlargement and contraction of the cavity of the thorax. The trachea is a tube composed of cartilaginous rings, united together by elastic ligaments, and furnished with muscular fibres, which commences in the fauces and descends into the thorax. It first divides into two branches, which pass respectively into the two lungs; here it is sub-

lin's experiments, and obtained the same results with respect to the oxygen and carbonic acid, but he conceived that they also consumed nitrogen; *Mem. sur la Respir.* p. 184. et alibi. He also details a variety of experiments, in which animals that possessed no distinct organs of respiration deoxidated the air in the same manner with those that have lungs; *Mem.* p. 258, 301. et alibi. We may infer that in all these cases the same kind of change is effected on the blood or other analogous fluid, for it is this change which is to be regarded as the ultimate object and essence of the function; *Magendie, Phys.* t. ii. p. 261. For a judicious summary of the various experiments that have been performed on the lower classes of animals, the reader is referred to the third chapter of Mr. Ellis's "*Inquiry*," and the additions to c. 3, in the "*Further Inquiries*."

divided into smaller and smaller branches, until it finally terminates in the air cells or vesicles. As the tubes become smaller, they gradually lose their cartilaginous nature, and are at length entirely composed of membrane. The muscular fibres of the trachea are placed both longitudinally² and transversely, and as the rings are incomplete at their back part, the tube easily admits of contraction in both its dimensions. A number of air vesicles, connected together by cellular texture, form what are styled lobules; a number of these lobules compose lobes, and a smaller number of these lobes constitute the lungs.

The pulmonary blood-vessels may be considered as forming the most essential part of the respiratory organs, or that to which all the rest are subservient. When the blood leaves the right ventricle of the heart, it is propelled through what has been called the *Rete mirabile Malpighi*, from its having been first described by this anatomist: the blood is then collected in the systemic veins, and is brought back to the left or systemic auricle of the heart. The lungs themselves are two masses of a spongy texture, which completely fill the cavity of the thorax; this cavity is lined by the pleura, and the lungs are also enveloped by a duplicature of the same membrane. In all the

² The most eminent anatomists admit of the existence of muscular fibres in both directions; Sabatier, however, informs us that he was never able to see the longitudinal fibres of this part; *Anatomie*, t. i. p. 261. Helvetius doubts of their existence altogether; *Mem. Acad. Scien. pour 1718*, p. 23, 4.

different states of respiration the two parts of the pleura, that lining the chest, and that enclosing the lungs, are in contact, no actual cavity being left between them.³

³ It was a prevalent opinion among the ancients that there was a quantity of air in the cavity of the thorax between the pleurae, and the opinion has been sanctioned by the authority of Harvey, *de Gener. Ex.* 3. p. 6; Hamberger, *Disp. de Respir. Mech.*, as referred to by Haller, *El. Phys.* viii. 1. 13; Hoadley, *Lect. on Respiration*, No. i. p. 11. et seq.; Hales, *Stat. Essays*, v. ii. p. 81; Morgagni, *Advers. Anat.* par. 5. § 46; and other eminent names among the moderns. See Boerhaave, *Prælect. t. v. pars. 1. notæ ad § 606*; Haller, *El. Phys.* viii. 2. 3., 8; and Dumas, *Physiol. t. iii. p. 40. et seq.* But the experiments of the later physiologists, and especially of Haller, *Op. Min. t. i. p. 301. 319*, appear to have decided this question in the negative. This writer, with his usual candour, states very fully the facts which have been adduced in favour of the contrary opinion; and at the same time, offers many important considerations, which may enable us to detect, as well as to obviate, the sources of error to which experiments on this subject are always liable. See also Whytt on *Vital Motions*, p. 81. note; Bichat, *Anat. Descrip. t. iv. p. 6*; and the same author in his treatise, *Sur la Vie, &c. Art. 6. § 1. p. 146, 7*; Sæmmering, *de Corp. Hum. Fab. t. vi. § 12. 16*; and Dumas, *Phys. par. 3. Sect. 2. c. 2. t. iii. p. 40. 5*. Although this point seemed to be so completely established, some experiments have been lately performed by Dr. Williams, a zealous and intelligent physiologist, which were attended with a different result; *Ann. of Philos. v. v. N. S. p. 429*. Notwithstanding the respectability of Dr. Williams himself and of the gentlemen who witnessed the experiments, I cannot but suspect that some circumstance escaped their observation, which has led to an erroneous conclusion. Some experiments not unlike Dr. Williams's were many years ago performed by Houston, *Phil. Trans. for 1736, p. 230*; See Hoadley's remarks upon them, *Lect. on Respir. Appendix*.

The diaphragm is a strong muscular expansion, possessed of a great degree of contractility,⁴ which separates the two principal cavities of the trunk of the body, the thorax and the abdomen. In its natural state it assumes an arched form, convex with respect to the thorax; but when it contracts, the curvature is necessarily diminished, and the thorax is of course increased in its capacity. The parietes of the thorax are composed partly of bone and partly of cartilage; the ribs, which form its sides, are arched bones, articulated at their extremities, and with spaces between each of them, that are occupied by muscles, called, from their situation, intercostals. When the ribs are in their natural position, and the muscles are relaxed, their lower edge forms an acute angle with the spine; but when they are raised by the contraction of the intercostals, they are more nearly at right angles to this bone, and thus contribute to enlarge the capacity of the thorax, although this effect is principally brought about by the contraction of the diaphragm.⁵

The mechanical act of respiration consists essentially in increasing the cavity of the thorax, which is accomplished principally by flattening the arch of the diaphragm; for although the contraction of the intercostals, by raising the ribs, tends to increase the dis-

* Haller in his experiments on the comparative contractility of various parts, remarks, "L'irritabilité du diaphragme qui paroît supérieure à celle des autres muscles;" *Sur des Part. Irrit. et Sens.* t. i. p. 257, et ex. 210, 225, 230, 239, 240.

† In Mr. C. Bell's *Dissect.* pl. 6, 7, we have an excellent view of the aspect and situation of the thoracic viscera.

tance from the sternum to the spine, yet the additional space gained in this way is but inconsiderable, when compared to that produced by the contraction of the diaphragm. Indeed it would appear, that the chief use of the intercostals is to fix the ribs, and thus to afford a kind of resistance to the power which the diaphragm would otherwise exert in drawing them down, and thus partially counteracting its own contraction.

As the lungs are every where in contact with the cavity containing them, their expansion must be always equal to that of the chest. The air which they contain, in consequence of this expansion, becomes rarefied; and as there is a free communication with the atmosphere through the trachea, a portion of air will enter the lungs, sufficient to restore the equilibrium. After some time the muscular contraction of the diaphragm and the intercostals ceases, and is succeeded by relaxation; the elasticity of the cartilages and membranes brings back the parts to their former shape, in which they are occasionally aided by the muscles of the abdomen and the loins,⁶

⁶ Sabatier, *Anat. t. ii. p. 274.* and Cuvier, *Leçons, t. iv. p. 357.* are, I believe, the only modern anatomists of eminence, who attribute expiration principally to the contraction of the abdominal muscles, and suppose the elasticity of the parts connected with the chest to be a secondary agent in this operation. Cuvier expresses himself very decidedly on the subject; expiration “*est due principalement aux muscles du bas-ventre, qui sont, à cet egard, les vrai antagonistes du diaphragme.*” Except in forced expiration I conceive these muscles to be nearly passive. We have some observations by M. Bourdon, on certain

and as the capacity of the lungs is thus diminished, a quantity of air is expelled from them. In a short time, however, the contraction is renewed, and is again succeeded by relaxation, and this alternation proceeds as long as life continues. From this account of the mechanical process of respiration we learn, that what may be called the quiescent state of the respiratory organs is expiration; that the air enters the lungs in consequence of the increased capacity of the chest, as affected by muscular contraction; that expiration is, in a great measure, a passive operation, and therefore that the act of inspiration is the one immediately connected with the powers of life, the remaining part of the mechanism of respiration depending principally upon the elasticity and other physical properties of the organs concerned.⁷

I have now described what takes place in an ordinary act of respiration; but although the function cannot be altogether suspended by any voluntary effort, it is so far under the control of the will, that,

points connected with the mechanism of the respiratory organs, which appear to be deserving of attention; they principally regard the state of the chest and its appendages during violent efforts of various kinds. These he conceives depend upon, or are always accompanied by, the closing of the glottis, which is essential to the action of the other parts of the operation; See an account of the work in *Med. et Phys. Journ.* v. xlv. p. 33. et seq.

⁷ Cullen's account of the mechanism of respiration in his "*Institutions*," sect. 3. c. iv. affords an excellent specimen of his clear and concise manner of handling a subject, which, by most of his contemporaries, was but imperfectly understood.

according to circumstances, it may be exercised in very different degrees. When we wish to make a full inspiration, besides the diaphragm and intercostals, we call into action the external muscles of the breast, shoulders, and other neighbouring parts, which, by elevating the ribs and the sternum, still farther increase the capacity of the thorax.⁸ When, on the contrary, we wish to produce a full expiration, the abdominal muscles are contracted, the viscera are thus pushed against the diaphragm, and its convexity towards the thorax is increased.

The above account of the mechanical process of respiration will, I conceive, be found sufficient to explain all the phenomena, without having recourse to any occult agents or any gratuitous suppositions, and is the one which is now adopted by the most judicious of the modern physiologists. But it was not until after many premature and imperfect attempts, nor without numerous and even violent controversies, that the correct theory was established.

⁸ An elaborate account of the effect of the contraction of these muscles may be found in Boerhaave, *Prælect. t. v. p. 1. § 613. 7*, and in Haller, *El. Phys. viii. 1. 17. 25*. The following authors may be also consulted on the mechanism of respiration; Borelli, *p. 2. prop. 81. 95*; Bellini *de Urin. et Puls. Introd. de Respiratione*; Senac, *Mem. Acad. pour 1724*; Winslow, *ibid. pour 1738, p. 65*; also *Anatomy, sect. 3. art. 13*; Dumas, *Physiol. par. 3. sect. 2. c. 4*; Magendie, *Physiol. t. ii. p. 267. et seq.*; the opinion of this last author differs considerably from that of Haller, on the motion of the ribs, and the share which they have in the increase or diminution of the chest.

While the physical properties of the air were little understood, it was natural that many errors should prevail respecting the action of the atmosphere on the human body, and while the abhorrence of a vacuum was assigned as the cause of many of the grand operations of nature, we cannot be surprised that it should be supposed to assist in respiration. Boyle appears to have been the first who explained upon correct principles the cause why the air enters the lungs; ⁹ but his simple doctrine was not relished in that age of refined hypothesis, where every thing was to be explained by some abstruse mathematical problem, so that for nearly a century after he wrote, a number of learned, but unfounded speculations, continued to prevail upon the subject.¹

An opinion which was supported by high authority, and had direct experiments adduced in its behalf, even by Hales,² was the existence of a quantity of air

⁹ He remarks that it had long been a subject of controversy, whether the organs of respiration acted like bellows, into which the air rushed because they are expanded, or like a bladder, which expands because the air is forced into it; he decides that the thorax acts like bellows and the lungs like a bladder. The lungs having no muscles, must necessarily be passive; the diaphragm is supposed to be the great agent in the expansion of the chest; Works, v. i. p. 101. See also Franc. de le Boe Sylvius, Opera, p. 16; Borelli, par. 2. prop. 82, 83; Mayow, Tract. p. 271, et seq.; Charleton, *Econ. Anim. Exercit.* 8. § 8. 11; Swammerdam, de *Respiratione*, sect. 1. c. ii.

¹ Baglivi, *Op.* p. 454; Hoadley's *Lect.* p. 12; Bremond, *Mem. Acad. pour 1739*, p. 333.

² *Stat. Essays*, v. ii. p. 81.

in the cavity of the chest, which by its elasticity compressed the lungs, and thus produced expiration. According to another opinion, at one time very prevalent, the lungs were furnished with a number of pores, through which a portion of the air passed and was again absorbed, in the different states of inspiration and expiration;³ but later observations have decided against the existence of these passages. Most of the older anatomists spoke of the lungs as possessing some kind of innate motion, by which they alternately drew in and expelled the air; but this opinion, although various experiments were adduced in its favour, has been generally discarded, as muscular fibres have not been detected in the lungs;⁴

³ Boerhaave, *Prælect.* t. v. par. I. notæ ad § 606; Hales is inclined to believe in the existence of these passages, *Stat. Essays*, v. i. p. 235.

⁴ Although the most accurate modern anatomists do not admit of the existence of muscular fibres in the lungs, some of the older physiologists conceived that they had demonstrated their existence; see Willis, *Pharm. Rat.* p. 9; Malpighi, in *Phil. Trans.* for 1671, p. 2150; Bremond, *Mem. Acad. pour 1739*, p. 338. et seq. It is to be presumed that these authors were misled by a false hypothesis, which caused them to believe that the lungs required a muscular structure in order to perform the function of respiration, and afterwards supposed that they were able to detect it. Darwin, as appears by certain passages in the *Zoonomia*, still adheres to the old opinion; v. i. p. 40; and v. ii. p. 50; but with all respect for the genius and literary merit of this writer, he possesses no authority on a question of anatomical fact. We find the same opinion also maintained by Dumas, *Physiol.* par. 3. sect. 2. c. 3. t. iii. p. 51. et seq. I am aware likewise that Reisseissen has announced the existence

and we are acquainted with no other method in which animal motion can originate.

When the lungs are removed from the body, and the trachea remains open, they generally collapse and are contracted into a smaller space than they occupied while in the cavity of the thorax. This has been ascribed to the re-action occasioned by the elasticity of their cartilaginous and membranous parts,^a which,

of muscular fibres connected with the bronchia; *Edin. Med. Journ.* v. xxi. p. 450; but it may be prudent to withhold our assent to the supposed discovery until it has been confirmed by other anatomists. See article "Poumons" in *Dict. Scienc. Med.* p. 512, 527, by Montfalcon. It may be necessary to remark that Haller applies the term "caro" to designate the substance of which the lungs are composed; *El. Phys.* viii. 2. 26; but it does not appear that the ancients, or the older of the moderns, intended to express by this word what we technically call muscular flesh: Pliny speaks of the "caro" of plants, *Nat. Hist.* lib. xvi. c. 38.

The diminution in the bulk of the lungs, when the thorax is laid open, has been always ascribed to their elasticity, but in their ordinary state, suspended as it were in a vacuum, and consequently having the pressure of the atmosphere removed from them, this power can only operate as an additional quantity of elasticity imparted to the parietes of the thorax. I conceive that the reasoning of Dr. Carson on this subject is not well founded. His experiments, made with a view to ascertain the amount of the elasticity of the lungs, he candidly confesses to be imperfect in the execution; nor do I think the mode which he employed for this purpose will be found competent to the end in view; *Phil. Trans.* for 1820, p. 29. et seq. When the lungs collapse they discharge a portion of air, this will pass into the globe which he employed, and expel a part of the water into the connected tube. But before we can obtain in

while in the body, had been retained in a state of over-distention, in consequence of the pressure of the internal air, not being balanced by any air between the pleuræ. Boerhaave and Haller attribute part at least of this effect to the contraction of the muscular fibres of the trachea and bronchia,⁶ but it must be observed, in opposition to such great authorities, that the contractile power of the lungs remains for a considerable time after their removal from the body, and cannot therefore depend upon a cause which must cease with the vitality of the part.

Few subjects in anatomy and physiology have caused more violent, and even acrimonious disputes, than the nature of the action of the intercostal muscles.⁷ Between each of the ribs are two distinct layers of muscular fibres, which are situated obliquely, but in an opposite direction, so as to decussate. It was the general opinion of the ancients, that the external layer, when it contracted, would raise the ribs, and consequently encrease the capacity of the thorax, but that the internal intercostals would depress the ribs, and of course diminish the size of the chest.

this way a measure of the elastic force of the lungs, we must ascertain the proportion which their bulk bears to that of the globe, and this to the quantity of air expelled from the lungs. Dr. Elliotson has inadvertently attributed the collapse of the lungs, when the thorax is opened, to the same cause which produces ordinary expiration; *Trans. of Blumenbach*, Note B. p. 82.

⁶ Boerhaave, *Prælect.* § 602. et notæ.

⁷ See Haller, *El. Phys.* viii. 1. 13; Dumas, *Physiol.* par. 3. sect. 2. c. 4.

Mayow, who in so many respects outstript the science of his contemporaries, and whose works afterwards became so remarkably neglected, appears to have been the first who adopted the opinion, that both sets of intercostals, by their contraction, must raise the ribs, and thus encrease the size of the thorax.⁸ But his experiments and reasoning were either not attended to, or failed in producing conviction, for the old opinion appears to have been generally entertained, until the middle of the last century. The doctrine of Mayow was, however, zealously embraced by Haller,⁹ and since his time has been, for the most part, acquiesced in.¹ But it is generally supposed that, in

⁸ Tract. p. 278. et seq. It is stated by Winslow, Mem. Acad. pour 1738, p. 92. and by Haller, El. Phys. 8. 1. 14. that Fabricius had previously announced the same opinion, but by referring to his treatise De Respiratione, p. 176, 7, it appears that he unequivocally supports the old doctrine.

⁹ Boerhaave, Prælect. t. v. par. 1. § 613 et notæ; Haller, El. Phys. viii. 1. 12. et seq. et viii. 4. 9.; an account of his experiments on the subject is contained in his Op. Min. t. i. p. 270.. 293. Hoadley, Lect. on Respir. p. 5..8, and Hamberger were among the most zealous defenders of the old doctrine; the latter appears to have maintained it with a degree of vehemence quite disproportioned to the importance of the object, if we may rely upon the complaints of Haller, El. Phys. viii. 1. 13. Sabatier takes a directly opposite view of the subject, and supposes that both sets of intercostals will have the effect of depressing the ribs; Mem. Acad. pour 1778, p. 347; also Anat. t. iii. p. 469.

¹ It would appear that Scemmering still entertains some doubts respecting the action of these muscles: after describing the internal intercostals, and stating that their effect will be the same with that of the externals, viz. to raise the lower towards

ordinary respiration, the intercostals are not much employed, except for the purpose of fixing the ribs, and that it is only in cases of violent action of the respiratory organs, or where, from accident or disease, their ordinary action is impeded, that these muscles have any effect in encreasing the size of the thorax.²

The nature of the diaphragm, its muscular action, and its importance in the mechanism of respiration, were but imperfectly understood by the ancients. By

the upper ribs, and consequently to dilate the chest and serve for inspiration, he asks, "an costas deprimunt?" *Corp. Hum. Fab. t. iii. p. 177.* Murat, also, the writer of the article "Intercostals," in the *Dict. Scienc. Med.* conceives, that although the effect of both sets of intercostals must be the same, and that this generally is to raise the ribs, and consequently to expand the chest, yet that under certain circumstances, as where the false ribs are fixed by the action of the abdominal muscles, the contraction of the intercostals must depress the ribs, and thus contract the chest. The present Prof. Monro has committed a singular oversight in asserting that his "Father discovered, that both strata," the external and internal intercostals, "are subservient to the elevation of the ribs;" *Elements v. ii. p. 9.*; an oversight the more remarkable, as in a previous passage of the same work, *v. i. p. 371.*, he had correctly attributed the discovery to Mayow.

² The older anatomists were aware of the existence of cases, where the cartilages of the ribs were ossified, so as to prevent the action of the intercostals, without any material impediment to respiration. Winslow *ubi supra*; Fabricius de *Respir. c. 10. sub finem.* For farther information on this subject the reader may consult Borelli, *par. 2. prop. 81. 95.* Bellini, *lem. 11.*; Senac, *Mem. Acad. pour 1724*; Winslow, *idem pour 1738*; also *Anat. Sect. 9. § 6*; Boerhaave, *Inst. § 615*; ditto *Prælect. passim*; Haller, *El. Phys. lib. viii. passim*; Dumas, *Physiol. par. 3. § 2. c. 4*; Richerand, *Physiol. p. 199. et seq.*

some it was supposed to possess a kind of independent life, by others it was thought to be the seat of the soul, and it was generally regarded as possessed of some very mysterious or inexplicable power, until Fabricius, at the beginning of the seventeenth century, explained its action and properties upon correct principles.³ It is now universally regarded as the great agent by which the size of the cavity of the thorax is regulated; in its natural or relaxed state, it is arched up, so as to diminish the capacity of the chest, while this is necessarily encreased, when it is flattened by the contraction of its muscular part.⁴

Malpighi, to whose researches we are indebted for our knowledge of so many parts of minute anatomy, appears to have been the first who described the structure of the apparatus by which the air is distributed through the lungs and is enabled to act upon the blood,⁵ and the description which he gave of the parts has been generally supposed to be correct. Succeeding writers, as is too frequently the case, while they have professed to adopt the ideas of their predecessors, have indulged their imagination in inventing a disposition of the parts, which cannot be found in

³ De Respiratione, lib. ii. c. 8.

⁴ For an accurate description of the diaphragm and its action and uses, as well as for a very copious list of all that had been published concerning it before his time, Haller's treatise in his *Op. Min. t. i. p. 249*, may be consulted, as also his experiments on the motion of the diaphragm in living animals. *Ibid. p. 293. . 300. et Mem. sur les Part. irrit. et sens. t. i. p. 65. et seq.* See Albinus's *Tab. musc. No. 14. fig. 4. 5. 6. 7.*

⁵ *Epist. de Pulmonibus, i.*

the original account of them, and which has probably no actual existence. Willis, for example, gives a figure of a portion of the lungs, according to which they consist of a congeries of rounded vesicles, separated from each other, and every one of them provided with a distinct tube, so as to resemble a bunch of grapes,⁶ a structure which has probably no existence.⁷ On the other hand, Helvetius endeavoured to prove that the bronchia terminate in a cellular or spongy tissue, composed of a membranous substance, the cells of which have no determinate figure or regular connexion with each other.⁸ But, upon the whole,

⁶ Pharm. Rat. p. 2. tab. 3. fig. 1. Cheselden, Anatomy, p. 173, remarks upon Willis's description, that it is "imaginary and false, as he could not but have known, if he had ever made the least inquiry into the lungs of any animal."

⁷ Malpighi's description of the parts is as follows: "Diligenti indagine inveni totam pulmonum molem, quæ vasis excurrentibus appenditur, esse aggregatum quid ex levissimis et tenuissimis membranis, quæ extensæ et sinuatæ pene infinitas vesiculas orbiculares, et sinuosas efformant, veluti in apum favis alveolis ab extensa cera in parietes conspiciuntur." "Membranæ istæ vesiculæ videntur efformari ex desinentia trachææ, quæ extremitate, et lateribus in ampullosos sinus facessens, ab his in spatia, et vesiculas inæquales terminantur." See plate to the Epist. de Pulm.

⁸ Mem. Acad. pour 1718. p. 18. Helvetius appears, however, to have been influenced in his opinion by the texture of the lungs in the amphibia, which, as they differ from the human in the relative sizes of the component parts, may do so likewise in the connexions of these with each other. An account of the characteristic difference between the lungs of the warm and the cold-blooded animals may be found in Shaw's Lectures, v. ii. p. 2, 3.

the most probable opinion seems to be one of an intermediate nature, that there are separate groups of cells, which are connected together, while these groups are themselves distinct.⁹

⁹ Haller's general description of the lungs is contained in *El. Phys.* viii. 2. 9. . 18; his account of their minute structure, *ibid.* 26. . 30. He gives an ample detail of the controversy concerning the question, whether air that is impelled into the bronchial tubes, can pass into the intervals between the lobules, and the reverse; the authorities as to the fact appear to be nearly balanced, § 26; but it may be suspected, that when the transmission does take place, it is in consequence of the rupture of a portion of the delicate cellular membrane. Haller himself inclines to the opinion, that the cells do communicate, but that there is no communication between the lobules, § 30. With respect to the figure of the cells, Hales says that they appear in the microscope to be spherical; *Stat. Ess.* v. i. p. 241. *Monro Secundus* supposed that the lungs are composed of cells, in which the bronchia terminate, but that the cells communicate with each other, and that this is likewise the case with the lobules; *Elements of Anat.* v. ii. p. 89. et seq. See also *Sprengel, Inst. Med.* lib. 4. c. 4. sect. 1. p. 450, and *Boyer, Anat.* t. iv. p. 263, who appear to adopt the opinion of Haller. For what may be regarded as the current opinion among the French respecting the structure of the lungs, see the 4th vol. of *Bichat, Anat. Descrip.* p. 69, written by *Buisson*, who supplied the last portion of the work, which was left imperfect by the premature death of his preceptor: also the article "Respiration," p. 13, by *Chaussier* and *Adelon*, in the *Diet. Scienc. Med.* published in 1820; also *Dumas, Physiol.* p. 3. § 2. c. 2. p. 45. . 50. *Sæmmering, Corp. Hum. Fab.* t. vi. § 14, supposes that the individual air cells are separate from each other, but that they all communicate by the bronchial tubes; *Magendie*, like *Helvetius*, conceives that the lungs are composed of a spongy substance, the cells of which freely communicate with each other; *Physiol.* t. ii. p. 262. et seq.; *Journ. Physiol.* t. i. p. 79; and *Richerand, Physiol.* p. 206,

But whatever may be our opinion respecting the precise form of the vesicles, or the mechanical connexion which there is between the air cells and the blood-vessels, we know that the parts are so arranged, as to enable the air and the blood to act upon each other, by the blood being divided into a great number of small portions, and thus expose as large a surface as possible, and by being separated from the air merely by the interposition of a very delicate membrane.¹ The extent of the surface of the membrane lining the cavity of the air vesicles, must necessarily be very considerable; but with respect to the estimates which have been made of it by Keill,²

says, that most anatomists adopt the opinion of Helvetius, an assertion which appears to be scarcely warranted. Blumenbach considers the cells as being unconnected with each other; *Physiol.* § 139; and this appears to be the opinion of Cuvier; *Tabl. Elem.* p. 41, 2. For a very complete investigation of the structure of the lungs we are indebted to Reissessen, who seems to have examined the parts with the greatest minuteness. He describes the vesicles as the closed terminations of the bronchial tubes, possessing a cylindrical and somewhat rounded figure; he states that they do not communicate with each other, or with the cellular substance in which they are enveloped. *Edin. Med. Journ.* v. xxi. p. 448. et seq. There is a peculiarity in the circulation of the lungs, which appears to have been first announced by Reissessen, and is confirmed by the present Prof. Monro, *Elements of Anatomy*, v. ii. p. 96, that there is a direct communication between the bronchial artery and the pulmonary vein, so that the greatest part of the blood which is conveyed to the lungs by the former vessel is retained by the latter; *Edin. Med. Journ.* v. xxi. p. 454. et seq.

¹ Cuvier, *Lec. d'Anat. Comp.* t. iv. p. 298.

² Tent. *Med. Phys.* p. 80.

Hales,³ and other physiologists of the last century, there is reason to suppose that they are, in a great measure, imaginary; nor do we appear to have any data from which we can form a more correct conclusion.⁴

Many attempts have been made by physiologists to ascertain the quantity of air taken into the lungs by a single inspiration. All, however, that we can obtain on this point is the average quantity; for, as was remarked above, the action of the chest is so far under the control of volition, that we are able to receive into it at pleasure very different quantities of air. There is also a considerable difference in different individuals with respect to the size and form of the chest, and it is also probable that peculiar states of the constitution, and perhaps even

³ Stat. Ess. v. i. p. 241.

⁴ An interesting account of the comparative anatomy and mechanism of the respiratory organs in the five classes of the mammalia, birds, amphibia, fishes, and insects, is given by J. Bell, *Anat.* v. ii. p. 133. 168. It is written in that interesting and impressive manner, which is so characteristic of his works, although, in certain points, it is not technically correct. We have a very valuable article on the respiration of birds in Rees's *Cyclop.*; see also Hunter on the same subject in *Phil. Trans.* for 1774, p. 205, et seq.; he endeavours to prove that birds possess a proper diaphragm, but unless we use the word quite in a technical sense, it appears not proper to apply this term to any part of their thorax. Cuvier's "*Leçons*," on this, as well as on every other subject on which he treats, cannot be too carefully studied; t. iv. leç. 26. *passim*, and leç. 27. sect. 2; also Blumenbach's *Comparative Anatomy*, c. 14. with Mr. Lawrence's valuable notes.

particular habits, may have an effect upon the quantity of air received into the lungs. And besides the question respecting the average bulk of a single inspiration, there are three others connected with it, that are both curious and important. We may inquire, first, what is the quantity of air left in the lungs after an ordinary inspiration, which may be considered as the natural or quiescent condition of the thorax; second, what farther quantity we are able to expel by the greatest voluntary exertion; and, lastly, what quantity is still left in the lungs after the most complete expiration.

With respect to the bulk of an ordinary inspiration, the first writer who attempted to ascertain this point by experiment appears to have been Borelli;⁵ the method which he employed was afterwards improved upon by Jurin, who obtained results which would seem to be nearly correct. By breathing into a bladder, and making the necessary allowance for temperature and pressure, he estimated that he took into the lungs about 40 cubic inches.⁶ Since his time many attempts have been made to solve the problem, and results have been obtained which vary from a few inches to above 50. Goodwyn bestowed much attention upon the point, but his apparatus, although more complicated than Jurin's, does not appear to have been capable of furnishing

⁵ *De Motu Anim.* p. 2. prop. 81.

⁶ *Phil. Trans.* No. 355; v. xxx. p. 757, 8; La Motte's *Ab. of Phil. Trans.* v. i. p. 415.

equally accurate results, at the same time that his estimate involves some physiological positions, which are at least very questionable. His apparatus consisted of a closed vessel, provided with two tubes, through one of which he inspired, while the other terminated in a second vessel containing water; when he inspired from the closed vessel, an equal bulk of water was drawn into it from the second vessel, and by weighing the first vessel before and after the experiment, the weight of water raised, and consequently the bulk of air displaced, was ascertained.⁷ By taking the average of 30 inspirations, he concluded that the bulk of air received in the ordinary action of the lungs is no more than 14 cubic inches.⁸

But independently of other considerations, there are two obvious objections to this method of ascertaining the bulk of a single inspiration; first, that in breathing from the closed vessel, the water being raised from the open vessel contrary to its specific gravity, a greater effort would be necessary to receive the due quantity of air into the lungs; and although Goodwyn was aware of this circumstance, and attempted to obviate it,⁹ the nature of the apparatus seems to render this impossible. In the second place, as was remarked by Menzies,¹ when the mouth is removed from the tube, the external air will imme-

⁷ *Connexion of life with respiration*, p. 28.

⁸ *Ibid.* p. 36.

⁹ *Essay*, p. 32, 3.

¹ *On Respiration*, p. 18, 9.

diately rush into the closed vessel, and drive back, into the open vessel, a part of the water which had been raised from it, so to lead the operator to under-rate the volume of air taken into the lungs.

It will not be necessary to enter into an account of the various experiments which were subsequently performed upon this subject, because they may be regarded as, in a great measure, superseded by those of Menzies, which appear entitled to the greatest confidence, both from the nature of the apparatus, and from the uniformity of the results. He employed an allantoid, of the capacity of 2400 cubic inches, to which was fixed a tube furnished with two valves, so that the air of inspiration was kept distinct from that of expiration. He breathed into the allantoid until it was filled, and found that each expiration was somewhat more than 40 cubic inches: he confirmed the result by attaching to the other tube a second allantoid filled with air, from which he inspired, and found, in like manner, that the amount of each inspiration corresponded nearly with his previous estimate of a single expiration. He then instituted a set of experiments of a different nature: a man was immersed above the chest in warm water, the top of the vessel being furnished with a tube, by the rising or falling of the water in which any alteration was rendered visible in the bulk of the body. After using every precaution to ensure accuracy, he had the satisfaction to find that his experiments remarkably corresponded with each other, and also

with those of Jurin, making the average bulk of a single inspiration, 40 cubic inches.²

The quantity of air emitted from the lungs being very much under the control of the will, it follows that we are still able to expel a considerable portion

² On Respiration, p. 24. . 30. Although in fixing the bulk of a single inspiration at 40 cubic inches, I have been principally guided by the experiments of Menzies, yet it may be proper to state that many other eminent physiologists have given their sanction to this estimate. This is the case with Sauvages, *Nosol. Meth.* t. i. p. 596; Hales, *Stat. Ess.* v. i. p. 243; Haller, *El. Phys.* viii. 4. 6; Chaptal, *Chem.* v. i. p. 133; J. Bell, *Anat.* v. i. p. 193; Sprengel, *Inst. Med.* t. i. p. 470; Sæmmering, *Corp. Hum. Fab.* t. vi. § 65; Ellis, *Inquiry*, p. 104; and Monro, (*Tert.*) *Elements*, v. ii. p. 98. Richerand also estimates it at between 30 and 40 cubic inches, *Physiol.* by Delys, p. 206; Fontana at 35, *Phil. Trans.* for 1779, p. 349; and Mr. Dalton at 30, *Manch. Mem.* v. ii. 2d ser. p. 26. There are, however, some great authorities among the moderns, who have formed a different conclusion; but, for the most part, it appears to have been deduced from inconclusive reasoning, or from experiments which involve some doubtful or obviously incorrect principle. Sir H. Davy states, as the result of direct experiment, the bulk of a single inspiration to be only 13 cubic inches, *Researches*, p. 433; while in another part of his work he indirectly estimates it at 17 cubic inches, p. 410; Jurine of Geneva, supposes it to be 20 inches, which Hallé, his editor, considers too large a quantity, *Encyc. Meth. Art. Medecine*, t. i. p. 494; Mr. Kite fixes the quantity at 17 cubic inches, *Essays*, p. 47; Mr. Abernethy at 12, *Essays*, p. 142; and Delametherie at even a smaller quantity, *Journ. Physique*, t. xlv. p. 108. Messrs. Allen and Pepys inform us that the operator whom they employed in their experiments took in $16\frac{1}{2}$ cubic inches at an easy inspiration, *Phil. Trans.* for 1808, p. 256. The estimates that were formed by the older writers may be found in Haller, *El. Phys.* viii. 4. 6.

after an ordinary expiration. The amount of this has been variously estimated, and it probably differs much in different individuals; from the average of some trials which I have made upon myself and others, and from the statements which have been given by different authors,³ I think it may be fixed at 160 or 170 cubic inches, so to give 200 or 210 cubic inches as the difference between the states of ordinary inspiration and of forced or extraordinary expiration. It may seem not a little remarkable, that some physiologists, who have written expressly upon the functions of the lungs, and even upon the mechanism of respiration, have entirely overlooked this quantity, and have estimated the bulk of the thorax, by adding together the amount of an ordinary expiration with what is left in the lungs after the most complete act of expiration.⁴

From the formation and structure of the lungs it is, however, obvious that after the most complete and powerful act of expiration, we are still unable entirely to empty the thorax; and many experiments have been made, to ascertain what is the amount of this residual quantity, or what is the bulk of air which is still left in the lungs. The first experiments

³ Jurin fixed this quantity at 220 cubic inches, *Phil. Trans.* v. xxx. p. 758; La Motte's *Ab. of Phil. Trans.* v. i. p. 415; Menzies, p. 31; and J. Bell, *Anat.* v. i. p. 193, at 70 inches; * Fontana appears to make it only 40, *Phil. Trans.* for 1779, p. 355.

⁴ Goodwyn, p. 36, 7; Kite's *Essays*, p. 47; Davy's *Researches*, p. 411.

on this subject, which are worthy of any particular attention, are those of Goodwyn. He remarks that an animal, immediately before death, produces a full expiration, and therefore, by ascertaining the capacity of the thorax in the dead subject, we obtain a knowledge of the quantity under consideration. As the diaphragm is the only part of the chest which remains moveable, he endeavoured to fix it by applying a firm compress about the upper part of the abdomen. An opening was then made into the thorax, and the lungs collapsing by their natural elasticity, expelled the air which they previously contained, and thus left a cavity between the pleuræ, which he filled with water. This water he conceived would exactly measure the space previously occupied by the air left in the lungs, and taking the average of four experiments, he concluded the quantity to be 109 cubic inches.⁵

⁵ P. 25, 6. Mr. Coleman, on the other hand, who performed experiments on dogs, for the express purpose of examining the state of the lungs, after hanging, found that they contained only a very small quantity of air. He supposes, that during the violent struggles which precede death, the animal still retains the power of expelling air from the lungs, while the pressure of the cord upon the trachea prevents any from being received; *On Respiration*, p. 96. . 8. In order to reconcile this apparent contradiction, Dr. Skey ingeniously suggested, that the lungs of a man would, as Goodwyn supposes, be in a state of complete expiration from the mental impression which he must experience; but that this would not take place in an animal that was unconscious of its fate. Yet this suggestion will scarcely remove the difficulty; for, in the three criminals examined by Goodwyn, the lungs would appear

There are, however, many objections against Goodwyn's method of ascertaining the capacity of the lungs after a complete expiration. Although his position is in the main true, that an animal makes a complete expiration before death, it will require many restrictions before it can be adopted as the basis of a physiological calculation. The mass of blood in the pulmonary circulation, the action of the respiratory muscles, and the state of the vital powers generally, at the moment immediately preceding death, may all be supposed to produce a considerable effect upon the size of the chest. Accordingly Goodwyn himself, when he examined the state of the lungs after hanging, found the residual quantity of air to be very much greater, amounting to 260 cubic inches.⁶ This difference he accounts for, upon the principle, that these individuals must have expired under the influence of fear, which always produces a deep inspiration; but it may be remarked, that in cases of natural death, the same feeling exists in a greater or less degree, and must therefore produce similar effects upon the capacity of the thorax. And farther, it would appear impossible, from the apparatus which Goodwyn employed, so to confine the chest, as that

to have been very nearly in their ordinary state of distention, while it may be presumed, that although immediately previous to execution, the thorax, through the effect of fear, might be enlarged to its utmost capacity, yet during the act of dissolution, which is by no means momentary, the same mechanical struggle must probably take place in man, as in another animal.

⁶ P. 26, 7.

when the water was admitted between the pleuræ, the diaphragm, or the parts connected with it, might not be displaced, or caused to protrude into the abdomen. Mr. Coleman also observes, that after a complete expiration, the diaphragm is raised as high as the fourth or fifth ribs, a situation in which it would be beyond the reach of any compression which could be exercised upon it by the bandage, and that the pressure of the water within the thorax would likewise cause the ribs themselves to descend, and consequently draw down the whole substance of the diaphragm.⁷

But there is a still more decisive objection against Goodwyn's estimates, that it proceeds upon the principle of a complete collapse of the lungs being produced by the admission of water into the cavity of the thorax.⁸ A considerable pressure would, no doubt, in this case be exercised upon the surface of the lungs, which will produce a material contraction of their bulk, but it cannot obliterate the cavities of the bronchial vesicles, an effect which must take place before all the air can be expelled. Both from the shape and texture of these organs, as well as from actual experiment, we know that scarcely any degree of external force can so far evacuate the lungs, as to render them specifically heavier than water, a state which it is difficult to produce, even by means of the air-pump.⁹ From

⁷ On Respiration, p. 89.

⁸ Essay, p. 24.

⁹ Boerhaave, *Prælect.* t. v. p. 2. not. ad. § 681; Petit, *Mem.*

these considerations we may fairly conclude, that Goodwyn's estimate of the capacity of the lungs after a complete expiration, is too low, and that we shall be in no danger of over-rating the quantity, if we suppose it to be 120 cubic inches.

The problem respecting the quantity of air left in the lungs after a complete expiration, has been also made the subject of experiment by Sir H. Davy and by Mr. Coleman. Sir H. Davy proved by a previous experiment, that when hydrogen is respired it undergoes no absorption or any chemical change, but is merely diffused through the air contained in the lungs.¹ He then inspired a quantity of this gas, after which he made a complete expiration; and having ascertained what proportion the hydrogen discharged bore to the other gases expelled at the same time, from the quantity of hydrogen still left in the lungs, he calculated what would be the total quantity of air in the thorax; and this, after making a due allowance for temperature, he estimates at no more than 41 cubic inches.²

This estimate differs so much from the result of direct experiment, and appears to be so incompatible with what we might suppose to be the case, from considering the anatomical structure of the thorax, that it is

Acad. Scien. pour 1733, p. 4; Allen and Pepys in *Phil. Trans.* for 1808, p. 269; and for 1809, p. 410, et alibi. Mr. Dalton, however, conceives that the air remaining in the lungs after a forced respiration "cannot be much, and that it is of little consequence to prove it exactly." *Manch. Mem.* v. ii. 2d ser. p. 26.

¹ *Researches*, p. 400..9.

² *Ibid.* p. 409, O.

impossible not to suspect some error or fallacy. And I conceive we may explain the difficulty by supposing, that the hydrogen was not uniformly diffused through the cavities of the lungs, a supposition which seems in itself reasonable, when we consider the minuteness and intricacy of the passages in which the air is lodged, and which is confirmed by the experiments of Jurine, of Messrs. Allen and Pepys, and of Mr. Dalton; Jurine received the air of a single inspiration in four different vessels, and found the four portions of air to exhibit different chemical properties, containing a greater proportion of oxygen and a less proportion of nitrogen, according to the order in which they were expelled from the lungs;³ and the same difference was detected by Messrs. Allen and Pepys, and by Mr. Dalton, with respect to the proportion of carbonic acid in the different portions of the air of expiration.⁴ And if this be the case with the air which exists in the lungs in the natural process of respiration, we may conclude that it is still more likely to be the case with any extraneous gas, which is forcibly received into them. Hence it seems fair to conclude, that in the experiments of Sir H. Davy, the hydrogen had not been mixed with the

³ Encyc. Method. Art. "Medecine," t. i. p. 494.

⁴ Messrs. Allen and Pepys found the first portion of the air emitted from the lungs to contain only 3, the latter part as much as 8 per cent. Phil. Trans. for 1808, p. 257. Mr. Dalton informs us that the first portion contained 3 per cent. of carbonic acid, and had lost $\frac{1}{4}$ per cent. of its oxygen, while the last portion contained 6 per cent. and had lost nearly 8. Manch. Mem. v. ii. 2d. ser. p. 25, 26.

air in the vesicles, in the same proportion as in the larger trunks of the trachea, consequently that he operated upon a gas containing an over-proportion of hydrogen, and has hence formed too low an estimate of the total contents of the lungs.

The method which Mr. Coleman pursued appears simple and direct, yet the conclusion which he formed is still more extraordinary. He instituted a series of experiments for the purpose of comparing the state of the lungs after drowning, with their natural condition, and he hence deduced the diminution which the chest experiences after a complete expiration. For this purpose he applied a ligature about the trachea of an animal which had been previously drowned, and after detaching the lungs from the chest, he pressed out all the air which they contained into an inverted jar of water. He then inflated the lungs, and ascertained the quantity of air which they were capable of containing when they were fully distended. The proportion which these two quantities bore to each other differed considerably in the different experiments; but the diminution was always very much more than could have been previously expected, even as much as 43 to 1.⁵ As the lungs completely fill the cavity in which they are contained, no reduction could take place in their capacity without a corresponding change in that of the thorax; yet it appears inconceivable that the thorax

⁵ On Respiration, p. 96.

can, by any process, be reduced to $\frac{1}{4}$ of its ordinary dimensions.⁶

It is not easy to account satisfactorily for the results of Mr. Coleman's experiments, but some circumstances may be pointed out, which will, I think, remove a part of the difficulty. When an animal is in the act of drowning, a sense of suffocation is experienced, which produces a violent effort to expire. The muscles that are connected with the chest are therefore brought into strong contraction, and the lungs are reduced to their smallest bulk; but relaxation quickly succeeds, and the elasticity of the chest brings it back to nearly its former dimensions. The external air is, however, unable to enter the lungs, and, consequently, the air still remaining in them, becomes highly rarefied; and, at the same time, a portion of the aqueous secretion, which lines their cavities, will be converted into the gaseous state. After a short time a second effort is made to expire, by which a portion of the remaining air, mixed with aqueous vapour, is discharged, in consequence of which the air still left in the lungs will be farther rarefied, and a still greater proportion of aqueous vapour formed. This process will continue until the contractility of the muscles is entirely destroyed,

⁶ See Borelli, par. 2. prop. 94. Haller, *El. Phys.* viii. 4. 11, conceives it impossible that the lungs could be contracted even to half their natural dimensions, unless they were removed from the chest and deprived of air by boiling. See references to note 9, p. 28, 9.

and we may presume that, at this period, nearly all the air originally contained in the lungs will be expelled, and its place occupied by the aqueous vapour. Now, according to the method in which Mr. Coleman performed his experiments, a great portion of this vapour would be condensed, the moment that the lungs, by being removed from the thorax, were subjected to the pressure of the atmosphere, and any part of it that remained, would be destroyed in passing through the water of the inverted jar. And besides the complete destruction of the aqueous vapour, a considerable proportion of the permanently elastic gas contained in the lungs would be carbonic acid, a part of which would also be absorbed in its passage through the water. It may be farther observed, as in the case of Goodwyn's experiments, that from the form and texture of the vesicles, and still more of the bronchia, it is not possible, by mere pressure, to expel all the air which they contain. It appears therefore evident, that Mr. Coleman has very much under-rated the capacity of the lungs in their state of complete expiration, although it is obviously impossible to ascertain the amount of the error.⁷

⁷ The same cause may probably operate, to a certain extent, in death, produced by any cause, except by hanging; that the violent effort to expire will expel a portion of air, the place of which will be partly occupied by aqueous vapour, and thus make the residual contents of the lungs appear too small. Messrs. Allen and Pepys estimate the quantity at 108 cubic inches; Phil. Trans. for 1809, p. 412; this, I conceive, from various considerations above stated, to be below the average. In

From the above data, which, although confessedly imperfect, are the best which we possess, we may form some approximation to the knowledge of the quantity of air contained in the lungs in their different states of distention. Assuming 170 cubic inches as the quantity which may be forcibly expelled, and that 120 will be still left in them, we shall have 290 cubic inches as the measure of the lungs in their natural or quiescent state; to this quantity 40 cubic inches are added by each ordinary inspiration, giving us 330 cubic inches as the measure of the lungs in their distended state.⁸ Hence it will appear that about $\frac{1}{3}$ of the whole contents of the lungs is changed by each respiration, and that rather more than $\frac{1}{3}$ can be expelled by a forcible expiration. Supposing that each act of respiration occupies 3 seconds, or that we respire 20 times in a minute, a quantity of air rather more than $2\frac{1}{3}$ times the whole contents of the lungs will be expelled in a minute, or about 4000 times their bulk in 24 hours. The quantity of air respired during this period will be 1,152,000 cubic inches, or about 666 $\frac{1}{2}$ cubic feet.

There are two curious subjects of inquiry, connected with the mechanism of respiration, which have abundantly exercised the genius of physiologists; what is the cause of the first inspiration in the newly-born infant, and what is the cause of the regular alternations of inspiration and expiration during the

another part, indeed, of their papers they state 141 cubic inches as the residual quantity; *Phil. Trans.* for 1808, p. 270.

⁸ See Sprengel, *Instit. Med.* t. i. p. 470.

remainder of life. The first of these queries was proposed by Harvey as a problem for the consideration of his contemporaries. "*Quomodo nempe embryo,*" he asks, "*post septimum mensem in utero matris perseveret? cum tamen eo tempore exclusus statim respiret; imo vero sine respiratione ne horulam quidem superesse possit; in utero autem manens ultra nonum mensem, absque respirationis adminiculo, vivus et sanus degat.*"⁹ The question may be stated more generally, why is the animal which has once respired, under the necessity of continuing the respiration without intermission, when, if the air had never been received into the lungs, the same animal might have remained for some time without exercising this function?¹

Many solutions were proposed of this problem, depending upon principles which are obviously erroneous, and are now totally discarded; but the hypothesis of Whytt deserves attention, on account of the reputation which it long maintained, in some of the most distinguished schools of physiology. He observes, that before birth the blood of the fœtus is properly elaborated by the mother, but that when

⁹ Exer. de Gener. p. 361.

¹ There is scarcely any opinion in physiology, however absurd it may appear at first view, which has not found some supporters, and accordingly attempts have been made to prove that the fœtus breathes whilst still in the uterus. See Boyle's Works, v. i. p. 110; Haller, *El. Phys.* xxix. 4. 54; also Whytt on Vital Motions, sect. 9. p. 111. 114, where the subject is very fully and satisfactorily discussed.

the communication is cut off, it becomes necessary for the young animal to produce the requisite change in its fluids by means of its own respiration. In furtherance of this end he supposes that, immediately after birth, an uneasy sensation is experienced in the chest from the want of fresh air, which may be regarded as the appetite for breathing, in the same manner as hunger and thirst are the appetites for food and drink. To supply this appetite, the sentient principle, with which the body is endowed, causes the expansion of the chest, in order to prevent the fatal effects which would ensue, were not the lungs to be immediately brought into action. This appetite for air is supposed to commence at birth, because, in consequence of the struggles of the foetus at this period, the circulation will be quickened, and an additional quantity of blood will now pass through the lungs, which stimulates them into action, and seems to be the immediate cause of this appetite. He considers the exercise of the function of respiration "as owing to a peculiar sensation of the body, which determines the mind or sentient principle to put certain muscles or organs into motion." With respect to Harvey's problem, he regards it "to be of so very easy solution, that it is not a little surprising, that many physiological writers should have attempted it in vain." He explains it upon the principle of the change which takes place in the direction of the blood, the whole of which now passes through the vessels of the lungs, and which would

stagnate in them, were it not propelled through them by the alternate motions of the chest.²

Haller refers the cause of the first inspiration to the habit which the fœtus had acquired, while in the uterus, of taking into the mouth a portion of the fluid in which it is immersed, and supposes that it still continues to open its mouth, after it leaves the mother, in search of its accustomed food; the air will therefore rush into the lungs, expand them, and thus reduce them to the state of a breathing animal, in consequence of which change they will require a regular supply of fresh air, to prevent the blood from stagnating in its passage from the right to the left side of the heart.³

A somewhat similar view of the subject is taken by Darwin. He coincides with Haller so far as to conceive, that the fœtus acquires the power of deglutition before it leaves the uterus; but he remarks, that the acts of swallowing and of breathing are essentially different. When the fœtus is separated from the mother, an uneasy sensation is experienced from the want of air; to remove this uneasiness all the muscles of the body are called into action, and among others those of the thorax, and the uneasiness being by this means relieved, to use his own expres-

² On Vital Motions, sect. 9. p. 109., 122. The author remarks that many physiologists have ascribed the first inspiration to instinct, but as he dislikes "the use of words whose meaning may be obscure or indefinite," p. 114, he prefers the explanation which is given in the text.

³ El. Phys. viii. 5. 2.

sion, "respiration is discovered," and the same action is afterwards repeated when the same uneasiness recurs.⁴ Dr. Philip thinks the difficulty may be solved by regarding the muscles of inspiration as entirely under the control of the will, and thrown into action by the uneasy sensation which the young animal experiences when it is separated from the mother, and can no longer have the necessary change produced upon the blood by her organs. He supposes the first inspiration to be entirely analogous to the first act of deglutition; the will, in both cases, causing the contraction of certain muscles for the purpose of removing an uneasy sensation.⁵ I shall not enter into a formal examination of these hypotheses; they appear to me to be built upon the assumption of principles, which are at least doubtful, if not altogether untenable; and with respect to the explanation offered by Whytt, it labours under the radical defect of all the metaphysical reasoning of the spiritualists, that it confounds the final with the efficient cause, and supposes the agency of an imaginary power, of the existence of which we have no evidence.

I think it may be doubted whether we are in possession of any data which will enable us fully to explain the difficulty, but there are some circumstances connected with the mechanical change which the lungs experience at birth, in consequence of the alteration of the position of the animal, that may throw some light upon it. Before birth the

Zoonomia, v. i. sect. 16. § 4.

⁵ *Quart. Journ.* v. xiv. p. 100.

lungs only receive one-third part of the quantity of blood which afterwards circulates through them,⁶ and are squeezed up into as small a space as possible from the posture of the foetus,⁷ as well as from the larger size of the heart and the liver, and by the thymus gland,⁸ so that the cavities of the vesicles and bronchia are nearly obliterated. The arch of the ribs is depressed, and the diaphragm is pushed up into the higher part of the chest, so that its concavity toward the abdomen is greater at this period than it ever afterwards becomes when the animal has once respired.⁹

As soon, however, as the position of the animal is changed upon its leaving the uterus, the trunk is extended, and the pressure removed from the thorax and abdomen. The elasticity of the parts being then at liberty to act, the arch of the ribs is raised, and the distance increased between the sternum and the spine, the liver and the other abdominal viscera now fall into their natural position, and permit the diaphragm to assume its ordinary curvature. All

⁶ Boerhaave and Haller, in *Prælect. t. ii.* § 200, cum notis.

⁷ Harvey de Gener. p. 353; Denman's Midwif. p. 218; Murat, in *Dict. Scien. Med.* art. "Fœtus," t. xvi. p. 55.

⁸ Haller, in Boer. *Prælect. t. v.* par. 2. not. ad § 681; and *El. Phys.* xxix. 4. 39; Denman, p. 158. 161; Blumenbach's *Physiol.* p. 361; Magendie, *Physiol. t. ii.* p. 435; Monro's *Elem.* v. i. p. 576; pl. 10; v. ii. p. 113.

⁹ Petit, *Mem. Acad. pour 1733*, p. 6; Senac, *ibid.* pour 1724, p. 171. See Hunter on the Gravid Uterus, pl. 12, 13, 20; also Sæmmering, *Icon. Embryon. Hum.* fig. 18, 19, 20; for the representation of the posture of the foetus.

these changes necessarily increase the capacity of the thorax, and cause the air to rush down the trachea of the animal into the bronchial vesicles, when the blood, meeting with less resistance to its passage through the lungs than through the foramen ovale, the whole of it passes through the pulmonary artery. The organs are thus brought into the state of ordinary expiration, or what I have termed their quiescent condition, when the necessity for inspiration will depend upon the same cause, which renders the alternation of inspiration and expiration essential to the future existence of the animal. According to this view of the subject, the first degree of expansion, which is produced in the lungs of the newly-born infant, depends merely upon the removal of external pressure, which permits the different parts of the trunk to assume their ordinary position. The farther increase of the size of the chest will depend upon the contraction of the diaphragm, and perhaps, strictly speaking, this contraction should be regarded as constituting the first act of inspiration.¹

¹ Besides the hypotheses of Whytt, Haller, and Darwin, which, in consequence either of their supposed merits, or the celebrity of their authors, have acquired some degree of consideration, many others have been formed by physiologists of eminence; as by Borelli, who resolves the question simply into the necessity which now exists for the young animal to perform those functions, which were before exercised by the mother, par. 2. prop. 118; by Pitcairne, Dissert. p. 62; and by Petit, Mem. Acad. pour 1733, p. 6, who refer it to certain general laws, which they suppose to prevail with respect to the action of the muscles and the animal spirits; by Lister, who explains it upon

Nearly allied to the question respecting the first commencement of respiration, is the inquiry into the cause of the regular alternations of inspiration and expiration, a subject which has given rise to as many hypotheses and speculations as the former, but being perhaps in itself more difficult of explanation, still remain at least equally involved in obscurity. Some physiologists have considered the necessity for the alternations of respiration to the support of life as a sufficient reason for its existence, thus substituting

the principle that the blood, which before birth passed through the umbilical, is now transmitted through the pulmonary vessels, *de Respir. in Exercit. Anat.*; by Swammerdam, who conceives that there is in the fœtus a space between the lungs and the thorax, which is filled with an aqueous vapour, which being expelled when the animal first attempts to breathe, enables the air to enter the lungs, *De Respir. Sect. 2. c. 1*; by Boerhaave, *Instit. § 691*; Hartley, on *Man, v. i. p. 95*; Buffon, *Nat. Hist. v. iii. p. 111*; and Blumenbach, *Physiol. § 151*, who ascribe it to the struggles of the fœtus when it leaves the uterus, by which the muscles generally, and the diaphragm in particular, are thrown into action, and the uneasy sensations which are experienced from diminished temperature, and the contact of surrounding bodies. Dr. Elliotson ascribes it solely to the impression of the cold air upon the surface of the body; *Notes to Blumenbach, p. 84*. Wrisberg appears to make no distinction between the cause of the first expansion of the chest and the subsequent act of inspiration; *De Respir. prima, in Sandifort, Thes. t. iii. p. 258.. 260*; Sprengel, *Instit. Med. t. i. p. 464*; and Parr, *Dict. art. "Fœtus,"* adopt an opinion very nearly similar to that in the text. Stæmmering, like Borelli, confounds the final with the physical cause; *Corp. Hum. Fab. t. vi. § 70*.

the final for the efficient cause of the action.² Others have attributed it to some mechanical effect, depending upon the pressure on the brain or a particular nerve, by the lungs or the diaphragm, at certain stages of the act of respiration.³ Others again have accounted for it by some speculative principle assumed concerning muscular contraction in general, which they have applied to the organs connected with the chest, among whom we may class Willis,⁴ Pitcairne,⁵ and Hartley;⁶ while others ascribe it to the effect of habit, association, or instinct.⁷

It will not be necessary to enter into any minute account of these hypotheses, and still less into any examination or refutation of them, as they appear to have been scarcely maintained except by the individuals who originally proposed them. But it may be proper to examine a little more in detail the opinions which were entertained upon this point by Haller and Whytt, because at one time they acquired considerable reputation, and probably approach somewhat more nearly to a correct view of the subject. Haller sets out with the position, that the passage of the blood through the lungs is impeded during expiration; this produces a reflux of blood into the veins,

² Borelli, par. 2. p. 117; Bellini, de Urinis, Intr. Lemma 18.

³ Boerhaave, Instit. § 419, O; Martine, Ed. Med. Ess. v. i. p. 156.

⁴ Pharm. Rat. par. 2. p. 18. ⁵ Dissert. par. 4. p. 62.

⁶ On Man, v. i. ch. 1. prop. 19.

⁷ See Sprengel, Inst. Med. § 210.

and causes a degree of pressure upon the brain. Hence arises a painful sense of suffocation, in consequence of which the will calls into action the muscles of inspiration, in order to enlarge the thorax, and, in this way, to remove the impediment. But the same uneasy feelings which were produced by expiration, ensue from inspiration, if too long protracted; the muscles therefore now cease to act, and by their relaxation produce the contrary state of the chest.⁸ Whytt, like Haller, conceives that the passage of the blood through the pulmonary vessels is impeded by expiration, and that a sense of anxiety is thus produced; this unpleasant sensation acts as a stimulus upon the nerves of the lungs and the parts connected with them, which excites the energy of the sentient principle, and this, by causing the contraction of the diaphragm, enlarges the chest and removes the painful feeling; the muscles then cease to act in consequence of the stimulus no longer existing.⁹

Upon these hypotheses we may remark, that they each of them involve three distinct positions, in the two first of which they agree, that during expiration, the passage of the blood through the pulmonary vessels is retarded, and that this produces a sensation of uneasiness in the chest; while they differ in the third position, the means employed to remove the uneasiness, Haller ascribing it to a voluntary effort, and Whytt to the operation of the sentient principle.

⁸ El. Phys. viii. 4. 17, 18, 28. and not. 6. ad Boer. Præl. § 619. t. v. p. 62.

⁹ Whytt on Vital Motions, sect. 8. p. 81, 109.

Now we may venture to affirm that all these assumptions are at least questionable, if not absolutely erroneous, so that notwithstanding the great names under the sanction of which they were advanced, we may pronounce the hypotheses to be untenable. Whytt and Haller have both fallen into the error of explaining the phenomena of natural or ordinary respiration by what occurs only in the unnatural or extraordinary efforts of the lungs. Now, as it will hereafter appear, it is doubtful whether the circulation through the pulmonary vessels be ever affected by the motions of the thorax, except in extreme cases of accident or disease, and although, in laborious respiration, we experience an uneasiness, which may prompt us to expand the chest; we are not sensible of this operation in ordinary cases, nor have we any evidence of its existence. Hence we must dismiss all those speculations which are founded upon the idea of the lungs having an appetite for air, analogous to the sensation of hunger, which is experienced by the stomach,¹ for it is impossible to conceive of an appetite which is unattended with consciousness or perception. And with respect to the third point of Haller's hypothesis, the interference of the will, we may remark, that respiration is performed in the most perfect manner by the infant immediately after birth, before any object of volition can be contemplated, or indeed before the faculty can have been called into existence; nor shall we be more disposed to place any

¹ Chaussier, Dict. Scien. Med. art. "Respiration," p. 23.

confidence in Whytt's doctrine of the sentient principle, as affording any actual explanation of the phenomena. I conceive therefore that we must consider this much agitated question as still undecided, nor do I think that we are in possession of any facts which will enable us to afford a satisfactory answer to it. I shall, however, offer some considerations, which may tend to show us towards what points our observations should be directed in order to obtain its solution.

When we reflect upon the intimate nature of the process of respiration, we shall find that our inquiry will assume the following form: what is the cause which produces the contraction of the diaphragm, when the same portion of air has remained for above a certain length of time in the lungs? and why, after a short interval, does this cause cease to operate, and permit the diaphragm to become relaxed? I am disposed to think that in the investigation of this subject, we must have recourse to three distinct principles of action corresponding to the different states of distention which the thorax experiences, or the different modes by which it is effected. A portion of air has been received into the lungs, and has acted upon the blood which circulates through them, and then becomes unfit for the farther performance of this office. Were the air not renewed, the blood, by not undergoing its appropriate changes, would be rendered incapable of carrying on its functions, one of the most important of which is to supply the muscles

with their contractile power; hence the heart would be no longer able to contract, and the circulation would cease. But at the very commencement of this state of the organs, or even probably before it has commenced, by a mode of communication, which we are perhaps unable to explain,² an impression is conveyed to the diaphragm, which causes it to contract, so as to enlarge the chest, and thus admit a portion of fresh air to enter the lungs. According to the usual operations of the animal economy, the impression, whatever may be its nature, no longer acting upon the diaphragm, its contraction is succeeded by relaxation, when the air, having now performed its due office, of rendering the blood proper for the support of life, is expelled, and the parts are brought back to their former state. The final cause of the operation is sufficiently obvious, and we are able to trace it through its successive steps; but we are not able clearly to understand the physical connexion between the state of the air and the contraction of the diaphragm, or rather, what is the exact nature of the stimulus which excites the nerves of this organ so as to cause it to contract.³

How far we can throw any light upon this train of actions, by supposing that the part of the eighth pair of nerves which is distributed over the lungs, imparts to them the power of receiving the perceptions of pain, which act as a stimulus upon the diaphragm, will be considered hereafter.

The diaphragm appears to be an organ which is singularly adapted for receiving the impressions of various stimuli, both on

The above remarks apply to what may be termed the ordinary act of respiration, where the effect appears to depend simply upon muscular contraction, although, as I conceive, we are unable to explain the nature of the stimulus which acts upon the contracted part, or the mode in which it is communicated to it. But there are other cases, in which the action of the respiratory organs seems to depend upon a cause of a different nature, where we are sensible of the existence of the stimulus, and are conscious of the effect which it produces upon the system. The chest is here thrown into a state of extraordinary action, from the more quick and violent contraction of the diaphragm, and the co-operation of various auxiliary muscles. In this case, of which sneezing may be adduced as an example, we are better able to explain the mechanical origin of the train of actions, while the interesting observations of Mr. Bell⁴ point out the nervous communication which subsists between the parts which connect the successive steps of the process, although it may be still somewhat difficult to explain the relation which it bears to the ordinary act of respiration.

Both of the above species of action are independent of the will, and, indeed, in the latter case, are often

account of its peculiarly contractile nature, which has been frequently remarked upon, and from the relations of its nerves, which, as Sprengel remarks, are connected with every part of the system; *Inst. Med. lib. i. cap. 4. sect. I. p. 443.*

⁴ *Phil. Trans. for 1821, p. 398. et seq. and for 1822, p. 284, et seq.*

in direct opposition to it; but there is a third species which is completely voluntary, as sighing, straining, &c. in which certain muscles are thrown into contraction, in order to accomplish some purpose, from a previous knowledge of their effects, in the same manner as the motions of the arms and legs. We may therefore conclude that ordinary respiration depends upon a cause, which, in some way that we are not able to explain, acts on the diaphragm, so as to produce its contraction; but when any circumstance requires the parts to be more fully expanded, it is either by the application of a stimulus to a sensitive part, which is connected with the respiratory muscles, or by the intervention of the will, according as the effect regards our immediate existence, or is only subservient to our comfort or accommodation.³

§ 2. *Mechanical Effects of Respiration.*

After explaining the mechanism of respiration, I proceed to consider its direct effects; these may be arranged under three heads, the mechanical effects, the effects produced upon the air, and those upon the blood. The older anatomists and physiologists insisted much upon the mechanical effects that were produced by the alternate motions of the thorax,

³ Magendie points out three degrees of inspiration, which he says are well marked, an ordinary, a great, and a forced inspiration, *Physiol.* t. ii. p. 274, but although they nearly correspond in their effects to what I have described above, they are supposed to depend either upon the action of different parts, or a different degree of action of the same parts.

upon the organs contiguous to it. Hales and Haller announced, as the result of direct experiments, made expressly for the purpose of ascertaining the point, that the circulation, and the other functions the most essential to life, were promoted or retarded according as the lungs were in the different states of inspiration and expiration, while they conceived that the due performance of the circulation and other vital functions was closely connected with the regular motions of the thorax. The parts or organs which have been supposed to be immediately influenced by the mechanical act of respiration are the following, the circulating system, especially the pulmonary vessels; certain nerves which are contiguous to, or in contact with the diaphragm; the abdominal viscera generally, and the liver in particular; and lastly, the lacteals and the thoracic duct.

As the earlier physiologists were entirely ignorant of the chemical effects of respiration, they were the more disposed to pay attention to those of a mechanical nature, and as their knowledge, even on this point, was very imperfect, we can scarcely be surprised at the errors which they committed. The intimate connexion which appeared to subsist between the respiration and the circulation induced them to examine minutely, whether the blood was transmitted through the lungs with equal facility during inspiration and expiration, and besides much hypothetical reasoning and elaborate calculation, numerous experiments were performed upon living animals to determine the question. We may, however, con-

clude that the mode of investigation which they adopted was quite inadequate to the purpose. The state into which the animal must necessarily be reduced, during experiments of this description, can at most only show us what takes place during the most violent or forced actions of the chest; the loss of blood, the pain inflicted, and the general derangement of the ordinary actions of the system could not fail to affect the organs of respiration so far as to render it impossible to learn, by this means, what effect their ordinary changes would produce upon the circulation and the other functions.

We may point out three distinct sources of error into which Hales and Haller, together with almost all the physiologists of the last century, fell, when they attempted to apply the results of their experiments to the question under consideration; first, they either entirely overlooked the unnatural situation in which the animals were placed, or did not make sufficient allowance for it; secondly, they observed the effects produced upon the circulation or other functions by the most violent efforts of the respiratory organs, and reasoned from these to their ordinary action; and, in the third place, they much exaggerated the change of bulk which the chest experiences in the different states of inspiration and expiration.⁶

We shall first examine how far the circulation is affected by the mechanical action of the lungs, as it

⁶ Haller, *El. Phys.* viii. 4. 11. and the authors to whom he refers.

was upon this function that the most important changes were supposed to be produced, and that to which the experiments were more particularly directed. The first experiments of this kind, which possess such a degree of precision, as to render them deserving of our attention, are those of Hales; he inserted a tube into the crural artery of a horse, and observed that the blood was evidently raised to a greater height in the tube, when the animal made a deep inspiration; this he attributed to the greater ease with which the blood was enabled to traverse the pulmonary vessels at this period, and thus furnish a more copious supply of it to the heart.⁷ Perhaps from this experiment we may go so far as to infer, that during a very full inspiration, and in an exhausted state of the system, the blood will be transmitted with more facility through the lungs. Yet we are not warranted to conclude from it that this would be the case under ordinary circumstances, while, at the same time, we may remark that Haller himself, who adopts Hales's conclusion,⁸ supposes that a very full and long protracted inspiration would retard the passage of the blood through the lungs.⁹

The opinion of many eminent physiologists was that, during expiration, when performed in its ordinary and natural manner, the bronchia and vesicles are, to a certain extent, folded up or compressed, so

⁷ Stat. Ess. v. ii. p. 6.

⁸ El. Phys. viii. 4. 11.

⁹ El. Phys. viii. 4. 13. Boerhaave also maintained this opinion, and founded upon it his hypothesis of the cause of the alternation of inspiration and expiration; Instit. § 619, O.

as to be less pervious to the blood, while, when they are expanded by inspiration, the blood is suffered to pass through them with facility.¹ This appears to have been the opinion which was generally entertained among Haller's contemporaries, and which was, by some of them, exaggerated in a most extraordinary degree.² This opinion professed to be directly deduced from experiment, but the experiments were of that vague and unsatisfactory kind, which were so frequently employed by the older physiologists, while no allowance was made for the peculiar situation in which the animals were placed, and

¹ Haller, *El. Phys.* vi. 4. 10; viii. 4. 11; viii. 5. 21. et alibi; notæ ad Boerhaave, *Prælect.* t. ii. § 200. The same opinion appears to be implied in the remarks of Scævenger, *Corp. Hum. Fab.* t. ii. § 68. Boerhaave is inclined to think that not more than one-third of the blood can pass through the lungs during expiration; ubi supra, not. 11. An opinion of a similar tendency had been previously maintained by Malpighi, *Oper. Post.* p. 19; and by Winslow, *Anat.* sect. 9. § 139. 1.

² J. Bell, *Anatomy*, v. ii. p. 188. et seq. in his usual impressive manner, very aptly exposes the extravagancies and inaccuracies of his predecessors on this subject, although his own opinions will perhaps be found not to be entirely correct. Harvey appears to be the first physiologist who clearly pointed out the independence of the actions of the heart and the lungs; yet he was of opinion that the passage of the blood along the pulmonary vessels is assisted by the motion of the thorax; *De Motu Cordis*, p. 11, 71, 77. Mr. Kite has given us a series of experiments the object of which is to prove, that in the state of complete expiration, which he supposes to be produced by drowning, very little blood can pass through the vessels of the lungs; *Essays*, p. 47. 58. But even if we admit his reasoning, it would not apply to the ordinary act of respiration.

for the consequent derangement which must necessarily ensue in every part of their œconomy. And we have, in support of the opposite opinion, an experiment of Goodwyn's, which appears to have been carefully performed, and which bears directly upon this question. He introduced a quantity of water between the pleuræ of a dog, and when even as much as one-third of the cavity of the thorax was filled, he could not perceive that the passage of the blood through the lungs was retarded, although the respiration was rendered laborious.³ The same view of the subject was also taken by Bichat, who has detailed various experiments and observations, made for the express purpose of ascertaining how far the circulation is affected by the state of distention of the lungs, the results of which appear to be quite decisive.⁴

It has been observed that when a portion of the cranium is accidentally removed, an alternate elevation and depression is visible, corresponding with expiration and inspiration,⁵ and Haller extends the

³ Essay, p. 45. Morbid cases of a similar nature, so far as this point is concerned, are not unfrequently met with, where fluids of various kinds are effused between the pleuræ, and where the respiration is much affected, while the circulation proceeds without any impediment. See Morgagni, on the Seats of Diseases, by Alexander, v. i. p. 408.

⁴ Sur la Vie, &c. part 2. art. 6. § 1.

⁵ Haller, El. Phys. vi. 4. 9; viii. 4. 27. et alibi; Mem. sur les Parties Irrit. et Sens. t. ii. p. 172, 192; see also the experiments of Haller's correspondents, Tossetti and Caldani, *ibid.* t. ii. p. 198. and t. iii. p. 141; with his own observations, t. iv.

operation to some of the other viscera.⁶ This effect has been attributed to the greater resistance which the blood experiences in its passage through the lungs while in the state of expiration; a stagnation is thus brought about in the right side of the heart, and consequently the blood is retained in the veins, which, in a highly vascular part, produces an increase of its bulk. When from the enlargement of the thorax the lungs become more pervious to the passage of the blood, the veins are enabled to empty themselves, and the increase of bulk is removed. It is probable that the phenomenon depends upon the cause that has been assigned; but it must be remarked, that in cases of this kind, where so considerable an injury is received by the cranium, or where any of the internal viscera are laid open, the respiration may be supposed to be more laborious than natural, so that the air will be taken in at

p. 15; also *Opera Minora*, t. i. p. 131, 7, 141. I had once an opportunity of witnessing this phenomenon, in a very striking manner, in a patient who had lost a portion of the cranium, owing to the growth of a fungous tumour; upon removing the tumour the bone was found to be in a diseased state, and was likewise removed, leaving the brain exposed. It would appear that the motion of the brain was observed by Lamure about the same time that Haller was engaged in his experiments on this subject. See Haller's Letter in *Op. Min.* t. i. p. 242; Lamure in *Mem. Acad. Scienc. pour 1749*, p. 541. et seq. His experiments were performed in the years 1751 and 1752, and were read to the Academy in 1752; the volume was published in 1753.

⁶ *El. Phys.* vi. 4. 9; *Op. Minor.* t. i. p. 137.

longer intervals and consequently in larger quantity, while the system being, at the same time, in an exhausted state, the circulation will probably be more easily affected by the operation of slight causes, which would not be perceived in its healthy and natural condition.⁷

But there is a consideration which is of more weight in the determination of this question, than the result of any experiments can be, in which it

⁷ The so much celebrated experiment of Hooke, in which, after the motion of the heart had ceased, it was re-produced by inflating the lungs, has been adduced to prove that the lungs are rendered more pervious to the blood by inspiration; Haller, *El. Phys.* viii. 4. 12. But it must be borne in mind, that in this case, the thorax was laid completely open, and that the lungs would consequently be collapsed into a much smaller bulk than while they were within the cavity of the thorax, at the same time that we account for the renewed motion of the heart upon a different principle, totally unconnected with a mechanical operation. With respect to the experiment itself, it is not a little remarkable, that although it excited so much attention when it was performed by Hooke, and was viewed by his contemporaries with almost childish astonishment, the very same process had been performed long before by Vesalius; see *Corp. Hum. Fab. lib.* 6. c. 19. in *Oper. t. i. p.* 571, 2. This great work was published in 1543; Hooke's experiment was performed in Oct. 1667. Hooke drew the correct inference from his experiment, and directly states his opinion that it was the fresh air, and not any alteration in the capacity of the lungs, which caused the renewal of the heart's motion. He farther informs us that the circulation through the lungs went on freely when the lungs were suffered to subside. For the original account of the experiment see *Phil. Trans.* No. 28. p. 539; see also Lowthorpe's *Ab. of Phil. Trans.* v. iii. p. 66; and Sprat's *Hist. of the R. S.* p. 232.

appears impossible to obviate every source of uncertainty. In the healthy state of the system we respire upon the average about twenty times in a minute, while the average velocity of the pulse may be estimated at eighty, so that the heart contracts four times during each act of respiration; and must consequently receive the blood during all the various states of distention to which the lungs are subject, yet we do not perceive that the pulse exhibits any corresponding variations either in its strength or its velocity. And farther, we shall find it extremely difficult to produce any effect upon the pulse by the most powerful voluntary efforts of inspiration or expiration; yet, in such cases, the capacity of the thorax will certainly undergo a much greater change, than it can possibly experience in its ordinary action.⁸

The connexion, which was supposed by the older writers to exist between the heart and the thorax depended upon their idea of the structure and mechanism of these organs, but some of the modern physiologists, who have insisted upon the reality of this connexion, have ascribed it to a cause rather of a metaphysical, than physical nature. Hunter speaks of a sympathy or association existing between the actions of the heart and the lungs;⁹ we also

⁸ This consideration appears to me, in a great measure, to counteract the force of M. Magendie's reasoning in a paper in his *Journal*, t. i. p. 132. et seq.; most of the experiments which he relates must be regarded as showing what takes place in an unusual action of the respiratory organs.

⁹ On the Blood, p. 54.

find an opinion of the same tendency advanced by Currie,¹ and Darwin still more explicitly refers to the effects of association. He says that "innumerable trains or tribes of other motions are associated with these muscular motions that are excited by irritation; as by the stimulus of the blood in the right chamber of the heart the lungs are induced to expand themselves, and the pectoral and intercostal muscles, and the diaphragm, act at the same time by their associations with them."² But, in opposition to this hypothesis, we may remark, that the lungs of a newly born animal act with full force immediately upon being placed in a situation where they can have access to the air, long before the effect of association can possibly operate. Besides, in after life, the periods of the contraction of the heart and the diaphragm bear no ratio to each other, the one being often much increased or diminished in frequency, while the other is not affected. The heart and the lungs are, however, indirectly connected with each other, inasmuch as they are both of them affected by the quantity and the quality of the blood which is transmitted through them, and, to a certain extent, are both of them under the influence of the nervous system. But this connexion is not to be referred to the effect of habit or association, but to the direct application

¹ Med. Reports, p. 77.

² *Zoonomia*, v. i. p. 40. Beddoes, proceeding upon this principle, goes so far as to form a project for rendering animals amphibious; *Observ. on Fact. Airs*, part i. p. 41. Upon this I shall have occasion to offer some remarks in a subsequent part of this chapter.

of the same agent to each separate organ. Upon the whole then we may conclude, that in the ordinary act of respiration the blood is transmitted through the lungs at all times with nearly equal facility, and that it is only in extreme cases that the retardation described by Haller and his contemporaries can be supposed to take place.

Besides the pulmonary system of blood vessels, there are other parts of the sanguiferous system which it was supposed would be affected by the mechanical actions of the thorax, and this it was conceived would be especially the case with the aorta and the vena cava, by the pressure of the diaphragm upon them during its contraction. Yet on inspecting the anatomy of the parts, it would scarcely appear that this effect should be expected to take place, as the passages through which these vessels are transmitted are guarded, as if for the express purpose of preventing the stagnation of their contents.³ Haller indeed informs us that, in some of his experiments, he observed the vena cava to be compressed by the diaphragm, so as perceptibly to impede the passage of the blood through it.⁴ Yet we may here, as on so many former occasions, attribute the effect to the state to which the animals were reduced, with all the functions deranged, the circulation probably much exhausted, and the diaphragm convulsed by the near approach of death.

³ Winslow's Anat. v. i. § 570. 572; Haller, El. Phys. viii. 1. 35; and Icon. Anat. fasc. 1; Bell's Anat. v. i. p. 326, 7.

⁴ El. Phys. vi. 4. 10; viii. 1. 36; viii. 5. 23.

Certain nerves, which perform very important functions in the animal œconomy, as, for example, the par vagum and the great sympathetics, pass through the diaphragm, and are so situated, that it was supposed they must be compressed by the contraction of this organ; it was therefore supposed that very important effects would hence result in the parts to which the nerves are sent, that there would be an alternate transmission and obstruction of the nervous influence, which would explain the motions of the chest, the pulsation of the heart, and the vermicular actions of stomach and intestines.⁵ But more accurate observations seem to prove that there is no foundation for this opinion, that we have no evidence for this pressure upon the nerves, and that we are able to explain the phenomena more satisfactorily upon other principles.

As the alternate motions of the chest, produced by the diaphragm, must cause a corresponding motion of the whole of the abdominal viscera; it has been thought that the agitation and pressure which they would hence experience will be instrumental in propelling the blood through the lungs, and in this way indirectly contribute to the formation of the various secreted fluids.⁶ But we may presume that this effect has at least been much over-rated; for it appeared in the experiments of Menzies, that the increase of bulk which the body experiences during

⁵ Martine, in Ed. Med. Ess. v. i. p. 156. et seq.

⁶ Haller, El. Phys. viii. 5. 23.

inspiration is precisely equal to the volume of air taken into the lungs,⁷ the change of capacity which the abdomen would experience from the contraction of the diaphragm being exactly balanced by the relaxation of the abdominal muscles, and *vice versa*, so that although the viscera may be supposed to be at all times under a certain degree of compression, the measure of it will be at all times nearly the same. No accelerations of the contents of the veins can therefore be produced by this cause, and indeed, as they are not furnished with valves, we may presume that any unusual pressure upon them would tend to diminish rather than to increase the flow of blood through them.⁸ Perhaps, however, we may, to a certain extent, admit of the mechanical effect of the respiratory organs upon the liver, in consequence of its situation with respect to the diaphragm and the ribs, and there may be supposed to be a peculiar necessity for some mechanical force of this kind with respect to the gall bladder, which is not furnished with muscular fibres, and would therefore appear to have no means of evacuating its contents, except what is derived from external pressure.⁹

⁷ Essay, p. 24. et seq.

⁸ Haller, El. Phys. iii. 2. 3; viii. 5. 23; C. Bell's Dissect. v. i. p. 48.

⁹ Haller, El. Phys. xxiii. 3. 29. Senac, in an elaborate essay on the diaphragm, in the Memoirs of the Acad. Scienc. for 1729, p. 118. et seq., supposes that the posterior muscles, or pillars, as they have been termed, when they contract, must obstruct the passage of the œsophagus, but this does not appear to be confirmed by subsequent observations.

It may seem also not improbable, that the state of compression in which the abdominal viscera are retained, and the alternate motion to which they are subjected, may have an effect in propelling the chyle along the lacteals and the thoracic duct. These vessels differ from the abdominal veins in the essential circumstance of being plentifully furnished with valves, and thus any increase of pressure, or perhaps even any change of position in the parts, which must be always attended with some partial compression, will have the effect of pushing forwards their contents. In the forced or extraordinary actions of the thorax, these effects may be even considerable, although we cannot suppose that they will be very powerful in ordinary respiration, and upon the whole we may conclude, that they are much less than was supposed to be the case by the physiologists of the last century.

§ 3. *Changes produced upon the Air by Respiration.*

From an inspection of the anatomy of the pulmonary organs, and particularly of the complicated apparatus by which the air and the blood are brought within the sphere of their mutual action, it was natural to conclude, that the principal use of the lungs is to produce some change in the blood through

¹ Senac, Mem. Acad. pour 1724, p. 173; Haller, El. Phys. xxv. 2. 6. Cruickshank supposes that the thoracic duct may be affected by the motion of the diaphragm in certain states of unusual contraction, but not by its ordinary action. On the Absorbent Vessels, p. 168, 9.

the medium of the air. Intimations of this kind are frequently met with, even in the writings of the ancients;² but it was not until comparatively of late years, that we obtained any clear conception of the nature or effect of this action. Perhaps the most generally received opinion among the earlier physiologists was, that the air, by being taken into the lungs, removes from the blood a part of its heat and water, for even the most superficial observer could not overlook the fact, that air which is expired from the lungs, differs from that which is taken into them, by the addition of warmth and moisture. During the reign of the mathematical sect, there was a great controversy respecting the question, whether respiration had the effect of rarefying or of condensing the blood. Those who supposed that a principal use of this function is the exhalation of water from the lungs, concluded that the blood must be condensed; while others, who conceived, that in the process of respiration a quantity of air is absorbed by the lungs, were equally strenuous in maintaining that the blood must be rarefied.

Although every one was aware of the necessity of the uninterrupted continuance of respiration, yet this was attributed more to some mechanical effect, which it produced upon the motion of the blood, than upon a change in its qualities: and it was not

² It has been remarked that Cicero, who borrowed his philosophy, both natural and moral, from the Greeks, speaks of the air possessing a vital spirit, but nothing is said about its nature or mode of operation; *De Nat. Deor. lib. ii. § 47.*

until the time of Boyle, that physiologists were fully sensible of the fact, that a perpetual supply of successive portions of fresh unrespired air is essential to life. Boyle discovered this fact in the course of his experiments with the newly invented machine, the air-pump; but as his ideas were principally directed to the investigation of the mechanical properties of the air, it was in this point of view that he almost exclusively viewed its action upon the lungs. Yet he was not altogether inattentive to the other changes which it experiences; he noticed the moisture which was exhaled along with it, and he further supposed that it carries off, what he stiles, recrementitious steams; but he does not give us any explanation of their nature. He also observed, that under certain circumstances, the air in which an animal had been confined for some time, was diminished in bulk; and this he accounts for by saying that it had lost its spring.³ Many of Boyle's contemporaries agreed with him in the opinion, that the blood parted with something to the air during its passage through the lungs; but there were, on the contrary, many eminent physiologists, and particularly among the chemists, who supposed that the blood acquired something from the air.⁴ This was

³ Boyle's Works, v. i. p. 99. et seq. and v. iii. p. 383.

⁴ We have a curious statement by Boyle, Works, v. i. p. 107, of a person named Debrelle, who is said to have contrived a submarine vessel, and also discovered a kind of fluid, which supplied the navigators with what was necessary for their respiration. Boyle is well known to have been too much dis-

the opinion of Sylvius,⁵ Baglivi,⁶ Borelli,⁷ Lower,⁸ and Willis,⁹ who imagined either that a portion of the whole mass of air, or that some particular part or element of it, entered the blood. Among these, the first in point both of genius and originality was Mayow, who investigated the nature of the change which the air experiences in respiration with peculiar felicity of experimental research and acuteness of reasoning. He announces the air of the atmosphere to be a compound body, and that it contained, as one of its constituents, a peculiar gaseous substance, which, from its supposed connexion with nitric acid, he termed nitro-aerial spirit. It was this nitro-aerial spirit which gave the air its power of supporting flame, and it was the same volatile spirit which imparted to the air its vital properties, and which the blood abstracted from it during its passage through the lungs. The hypothesis of Mayow seems to have attracted considerable notice at the time when it was proposed, yet it was shortly afterwards generally abandoned, and what is more remarkable, the experiments and discoveries of Mayow respecting the constitution of the atmosphere, which were extremely

posed to credulity; and we may venture to affirm, that the story, as he tells it, cannot be correct, yet it is scarcely probable that the whole was entirely without foundation.

⁵ Praxis Med. lib. i. c. 21. ⁶ Opera, p. 459, O.

⁷ De Motu Anim. pars 2. p. 113. ⁸ De Corde, p. 179. et seq.

⁹ Pharm. Ration. pars 2. p. 34. The great name of Newton may be added to this list, as we find that he supposed there was an acid vapour in the air, which was absorbed by the lungs during respiration; Opera, a Horsley, t. iv. Optics, p. 245.

curious, and admirably calculated to advance our knowledge of natural philosophy, fell into neglect, and at length were almost totally forgotten.¹

¹ The total neglect into which the experiments of Mayow had fallen, during the greater part of the last century, must be regarded as a very singular occurrence in this history of science. He lived at a period when experimental research was becoming fashionable, and had been sanctioned by many illustrious examples; his experiments were simple in their contrivance, and many of them very decisive in their results, and there does not appear to have been any circumstance in the mode of their publication or the situation of the author, which should have prevented them from meeting with due regard from the public. He appears indeed to have been pretty fairly estimated by his contemporaries, as we find his works referred to both by the English and French physiologists, and generally spoken of with a due degree of applause. See Lubbock, in *London Med. Journ.* v. i. p. 220. . 2. After an attentive perusal of his works, I consider myself justified in pronouncing Mayow to have been a man of extraordinary genius, and one who, on many points, far outstript the science of his age. Yet it must be confessed, on the other hand, that if, at one period, he was unjustly neglected, he has, at other times, been far too highly extolled. He saw the analogy of respiration to combustion, as well as the connexion which subsists between these processes and one of the constituents of the atmosphere, and he had also some conception of the modern doctrine of the formation of acids. But, I conceive, it would be no difficult task to prove, that on all these subjects, he had imperfect notions only, and that many of his ideas respecting the nitro-aerial spirit, as he terms it, are inapplicable to the modern oxygen, or to any other chemical principle. See note 30 of the *Essay on Respiration*.

In order to enable the reader to form a clear conception of the respective merits of our three countrymen, Boyle, Hooke, and Mayow, and of their claims to originality; I have subjoined the dates of some of their publications which give an

After the period of Mayow our knowledge respecting the change which the air experiences by respira-

account of the constitution of the atmosphere, and its effects on combustion, respiration, and other analogous processes. From these dates it may be fairly questioned, whether Mayow was always sufficiently candid in noticing the labours of his contemporaries. Boyle was born in 1627, and died in 1691; his principal works on the air were published between the years 1660 and 1675. Hooke was born in 1635, and died in 1702; he published the *Micographia* in 1665, and the "*Lampas*," in 1677. It is in the former of these works that he first detailed his ideas respecting combustion; *Obs.* 16, entitled "of Charcoal and Burnt Vegetables," p. 100. et seq. He commences the *Lampas* by referring to the *Micographia* as containing "his hypothesis of fire and flame, which," he adds, "has so far obtained, that many authors have since made use of it and asserted it," p. 1. See also Waller's *Life of Hooke* prefixed to his posthumous works, and Ward's *Lives of the Gresham Prof.* p. 190. Mayow was born in 1645, published his tracts in 1674, and died in 1679. The substitution of the year 1697 for 1679, in Mr. Brande's *Manual*, as the date of Mayow's death, is, no doubt, an error of the press; compare p. 64 with p. 69. With regard to the estimate of Mayow's philosophical character, although, in many respects, I have the satisfaction of coinciding with Mr. Brande in the high commendation which he bestows upon it, p. 68. 79; yet I cannot but think that he, like Beddoes and Dr. Yeats, has over-rated his merits; at the same time, I admit, that in the contrivance and the execution of his experiments, he appears to have been more successful than any of his contemporaries. See Beddoes's "*Chemical Experiments and Opinions*," &c. and Yeats's "*Observations*," &c. Robison strongly advocates the merits of Hooke against those of Mayow; *Black's Lectures*, note 13. v. i. p. 535. 8. and note 51. vol. i. p. 553. Fourcroy, I conceive, formed a more correct estimate of Mayow's merits; *Encyc. Meth. art. Chim.* t. iii. p. 390, and *Ann. Chim.* t. xxix. p. 42. et seq. Perhaps

tion was decidedly retrograde. Even Hales, who devoted so much of his time and attention to the investigation of the properties of air generally, and to the subject of respiration in particular, appeared to have no distinct conception of any proper chemical effect being produced by this function, a circumstance which is the more remarkable, as it would appear that he was not unacquainted with Mayow's discovery.² The result of Hales's experiments was that the air receives the addition of a quantity of aqueous vapour and certain noxious effluvia, and has its elasticity diminished.³ Boerhaave ingeniously confesses his ignorance on the subject,⁴ and Haller, after reviewing with his accustomed candour the opinions of his predecessors, coincides very nearly with the doctrine of Hales.⁵

It was about this period that Black commenced his chemical discoveries, one of the most important and best established of which was, that a quantity of what was then termed fixed air, or as we now more

one of the most striking proofs of the neglect into which Mayow's works had fallen about the middle of the last century is the circumstance of his discoveries on air not having been alluded to by Pringle, in his address to the Royal Society, on presenting Priestley with the Copley Medal; Discourses, No. 1.

² Statical Essays, v. i. p. 234. . 6.

³ Ibid. vi. ii. p. 94, 100. et alibi.

⁴ Prælect. t. ii. § 203. cum notis; t. v. § 625. cum notis et alibi.

⁵ Not. 40. ad Boer. Præl. t. v. § 625; El. Phys. viii. 3. 11, viii. 5. 19, O. et alibi.

correctly designate it, carbonic acid, is produced in the lungs, and that the air of expiration essentially differs from that of inspiration by the addition of this substance.⁶ This may be justly regarded as the first step towards a correct view of the nature of the function of respiration, and the brilliant career of discovery respecting the gaseous bodies, upon which Priestley was now entering, most fortunately contributed still farther to advance our knowledge.⁷ After he had made us acquainted with the nature and properties of oxygen, and shown that it is one of the constituents of the atmosphere, he instituted an extensive train of experiments for the purpose of ascertaining the state of the air under different circumstances in relation to the quantity of oxygen which it contains, and he found that respiration, in the same manner with combustion and other analogous operations, diminishes the proportion of oxygen,

⁶ Black's *Lectures* by Robison, v. ii. p. 87, 100, 204. Haller's third volume, which contains an account of respiration, was published in 1760; see a list of his publications, with their dates, prefixed to his *Op. Min.* p. xix. Black appears to have made his discovery about 1757, and we may presume that he shortly after announced it in his lectures.

⁷ Priestley's first account of his experiments and discoveries on the gases was read to the Royal Society in March 1772; and from the prodigious extent and originality of the matter which it contains, it necessarily follows, that his attention must have been, for some time, occupied with the subject, while from the peculiarly ingenuous and open disposition which he always manifested, we can have no doubt that, from time to time, he informed his numerous friends and correspondents of the success of his investigations.

and reduces the air to the state, which, in conformity with the hypothesis at that time generally adopted, was termed phlogisticated. Priestley's conclusion therefore was, that respiration deprives the air of a portion of its oxygen, and imparts to it a quantity of phlogiston and aqueous vapour.⁸

Shortly after the publication of Priestley's experiments, the subject of respiration was taken up by Lavoisier. Although this philosopher is perhaps less distinguished for his own discoveries, than for the acuteness which he displayed in generalizing and reasoning upon the discoveries of others, yet there is no individual to whom the science of chemistry is more indebted, from the minute attention to statical accuracy of which he showed the first example, and in which the experiments of the chemists of the present day so much exceed those of the most celebrated of their predecessors. He examined with his accustomed address the conclusion of Priestley; he agrees with him respecting the important fact of the consumption of oxygen, but he points out a distinction, which had been hitherto disregarded, between the various processes that had been classed together under the title of phlogistic; that some of them, as, for example, the calcination of metals, consisted merely in the abstraction of oxygen from the air,

⁸ Phil. Trans. for 1776, p. 147. In the first of his original volumes on air, published in 1774, he announced that air which had been respired was affected in the same manner as by putrefaction, and that one use of the blood is to carry off putrid matter from the living body, p. 78.

whereas in others, among which he classes respiration, we have not only the abstraction of oxygen, but the production of carbonic acid. His general conclusions therefore are, that the effect of respiration is to abstract oxygen and to produce carbonic acid; he further conceived that the total volume of the air is diminished, while the nitrogen he supposes is not affected by the process, but that it simply serves the purpose of diluting the oxygen.⁹ Lavoisier, in this memoir, does not notice the aqueous vapour which is discharged from the lungs, and which he afterwards made the subject of an elaborate train of experiments; we may therefore presume that, at this period, he considered it as merely diffused through the air by evaporation from the trachea and air vesicles, and not as formed by the operation of any chemical affinity.

The doctrine of Lavoisier concerning the chemical effects of respiration on the air was generally acquiesced in by the contemporary physiologists, at least in its more important parts, so that since his time, the attention has been principally directed to estimate the amount of the various changes which he

⁹ Mem. Acad. Scien. pour 1777, p. 185. et seq. It is impossible to omit noticing that in this paper Lavoisier makes no mention of Dr. Black, although he had publicly taught in his lectures for twenty years, that carbonic acid is produced by respiration. Nor is Black mentioned by Morveau in his account of the successive discoveries that had been made on the subject of respiration, given in the Encyc. Meth. Chimie, art. "Air," in the sect. on respiration, under the head "Acide Mephetique;" this was written in 1786.

supposed to take place. These I shall now therefore proceed to examine in succession, under the five following heads; the quantity of oxygen abstracted from the air, the carbonic acid formed, whether the volume of the air be affected by respiration, whether the nitrogen be affected, and lastly, I shall inquire into the origin and quantity of the aqueous vapour.

With respect to the first of the subjects, the abstraction of oxygen, besides the inquiry respecting the absolute quantity of it which disappears, there is another point which must be attended to, what proportion of it is it necessary for air to contain, in order to render it fit for the support of life. Although it might have been supposed that these questions would have been easily answered, by a sufficient number of well directed experiments; it appears in fact that this is not the case, for we find that there are many circumstances connected with the living body, and especially influencing the action of the lungs, which it is very difficult either to guard against or duly to appreciate. The capacity of existing in air of a certain standard, or of abstracting oxygen from the mass of air, depends very much upon the peculiar constitution of the animal as to its temperature and other functions, and still more, upon the structure of its lungs and organs of circulation. As a general principle it appears that animals which possess the highest temperature, whose lungs have their air cells the most minutely sub-divided, and the whole of whose blood passes through the lungs at each circulation, consume most oxygen,

require a greater proportion of it in the air which they breathe, and possess the power of deoxidating it in a less degree. This will appear to be the case if we compare together the four great divisions of birds, mammalia, amphibia, and mollusca: birds, whose temperature is upon the average about 104° , consume more oxygen, require a purer air, and less completely deoxidate it than the mammalia, whose temperature is about 98° , while the last, on all these points, are much exceeded by the cold-blooded animals.¹ So far as we know, the power of all the animals belonging to the great division of the mammalia is nearly the same, and is similar to that of the human species. This will, of course, be the main object of our investigation, although I may occasionally refer to the other classes of animals, either for the purpose of illustration, or where the facts or experiments are themselves so important, as to be an object of attention on their own account.

In forming an estimate of the absolute amount of oxygen consumed in respiration, there are many circumstances which tend to interfere with the results, and to render it difficult to obtain a correct estimate. Besides various points connected with the management of the apparatus, and the nicety of the manipulations, there is one of a physiological nature, which was first noticed by Crawford, was afterwards more fully developed by Jurine and Lavoisier, and

¹ Chaptal's Chem. v. i. p. 128. et seq.; Higgins's Minutes, p. 158, 9; Vauquelin, Ann. Chem. t. xii. p. 273. et seq.; Edwards, de l'Influence, &c. part iv. c. 7.

has been lately still farther investigated by Dr. Prout and Dr. Edwards. I refer to the fact, that the respiration of the same individual affects the air in a very different degree, according to the state of the functions and the system at large. The points which have been more particularly attended to are the temperature of the air respired and of the animal, the degree of muscular exertion, the state of the digestive organs, the presence of febrile action, and the hour of the day, to which some others may be added, probably of less moment. It is hence obvious, that all which we can accomplish in experiments of this kind is to obtain an average result, and even this subject to many modifications which must materially diminish the certainty of any conclusions that we form concerning it.

The first experiments on the actual amount of oxygen consumed in respiration, which were performed with any degree of accuracy, were two of Lavoisier's, in which he confined a guinea pig in pure oxygen, and afterwards in air containing a much greater proportion of it than exists in the atmosphere.² The gas was confined by mercury; in the first, the quantity was 248 cubic inches, and the experiment was continued during an hour and a quarter; in the second, which lasted for an hour and a half, he employed 1728 cubic inches; the animal

² Mem. Acad. Scienc. pour 1780, p. 401.. 8; Mem. Soc. Roy. Med. pour 1782, 3, p. 572.. 4; Ann. Chim. t. v. p. 261. et seq. This latter paper is written by Seguin, and contains a copious extract from the second of the above memoirs.

was then withdrawn, the bulk of the gas was noticed before and after the experiment; pure potash was introduced, in order to absorb the carbonic acid, and the bulk of the gas was again noticed. In the first case 100 parts of air were diminished 3.5 per cent. and of the 96.5 that remained, 16.5 were absorbed by potash, leaving a residuum of 80 per cent. In the second experiment, the diminution, in the first instance, was from 100 parts to 96.82, which, after the action of the potash, was farther reduced to 77.82. These experiments were probably performed with great care, and are highly interesting, as being among the first which this distinguished chemist performed on the subject of respiration; but owing to the peculiar circumstances under which they were made, they can scarcely be applied to what takes place in natural respiration; in the commencement of the experiment the animal was respiring an air considerably purer than that of the atmosphere, while, towards the conclusion, the reverse would take place.

In order to obviate this objection, Menzies proceeded upon the plan of examining the state of the air after it had been only once respired, when, by ascertaining what proportion of oxygen was abstracted, he calculated the total quantity which would be consumed in a given time. The result of Menzies's experiments was, that air, by being once respired, had $\frac{1}{10}$ part of its bulk converted into carbonic acid, from the known composition of which he estimated, that the quantity of oxygen consumed by a man in

24 hours would be 51840 cubic inches, equal to 17496 grs.³ This estimate of Menzies may be regarded as a considerable approximation to the truth, yet it depends upon several data of which only a vague average had been formed. It however deserves to be recorded, as the first experiment in which an attempt was made to estimate the amount of oxygen consumed by the human subject.

An elaborate train of investigation was conducted by Lavoisier, in conjunction with Seguin, upon whom the experiments were performed, in which an apparatus was employed more complete, and on a larger scale than any which had hitherto been introduced into physiological inquiries. One principal object of the experiments was to point out the effect which different states of the system produce upon the respiration, and they prove them to be much more considerable than had been previously suspected, while they show that we can do no more than obtain an average result, and that, after making many allowances for circumstances, the exact amount of which we are unable to appreciate. The general conclusion of Lavoisier was, that the average consumption of oxygen, during 24 hours, is 46048 cubic inches, equal to 15541 grs.⁴

³ Essay, p. 50; the weights as given by Menzies are 22865.5 grs. depending probably upon his having adopted a different number for the specific gravity of oxygen; I have followed the estimate given by Mr. Brande; Manual, v. i. p. 316.

⁴ An account of the operations of Lavoisier and Seguin is contained in two papers in the *Memoirs of the Academy of*

Lavoisier was still continuing his researches, and had constructed a new and more perfect apparatus, with which he had already performed some experiments, when he fell a victim to the barbarous fury of Robespierre. The results of his latest operations have been fortunately preserved, and are recorded by La Place; although on some points they differ from the former, yet they nearly agree with them in the amount of oxygen consumed in 24 hours, which they state to be 15592.5 grs.⁵

Since the death of Lavoisier this point has been made the subject of experiment by Sir H. Davy, who by adopting a plan nearly similar to that of

Sciences for the years 1789 and 1790, which give a detail of the results of two distinct sets of experiments, p. 566. et seq. and p. 601. et seq. But notwithstanding the apparent attention to accuracy in every part of the processes, the details do not, in all cases, correspond, nor is any reason assigned for their discrepancy. The weight of oxygen consumed is given very nearly the same in both the papers, in the first being 19080 French grains, in the second 19090. But the volumes are stated very differently; the first being 24 and the latter something more than 22 Paris feet. This difference, we may presume, depends upon different estimates of the specific gravity of oxygen. To make the numbers correspond, we must take the weight of 100 cubic inches of oxygen at 31.572 grs. in the first case, and in the second at 34 grs. after reducing the French weights and measures to the English standard; I have adopted the volume mentioned in the second experiment, as we may presume this was supposed to be a correction of the former. It must be remembered, that in these experiments, the measures are the direct results, and the weights estimated from the measures.

1 Suppl. to Enc. Brit. v. ii. p. 594.

Menzies, which consisted in ascertaining what proportion of its oxygen the air lost by being once respired, estimated the quantity of oxygen consumed in a minute at 31.6 cubic inches, which will give us 45504 cubic inches or 15337 grs. in 24 hours.⁶

In the *Philosophical Transactions* for the years 1808 and 1809, we have an account of a series of experiments which were performed by Messrs. Allen and Pepys, on the changes produced in atmospheric air and in oxygen by respiration. With respect to the correctness of the detail, and the minute accuracy with which the various chemical processes were conducted, it is impossible to speak too highly, but I think it may be questioned, whether the operators were equally fortunate in some of the mechanical parts. The apparatus consisted of a large water gazometer and a mercurial gazometer, which were so arranged, "that the inspirations were made from the water gazometer and the expirations from the mercurial gazometer alternately;" while, in order to keep the portions of gas separate from each other, it was necessary for the operator to open two cocks during each act of respiration, one connected with each of the gazometers. The individual on whom the experiment was performed began by making a complete expiration, and after having continued the process as long as was thought necessary, he concluded, by emptying the lungs as completely as possible.⁷

⁶ *Researches*, p. 431. . 4.

⁷ *Phil. Trans.* for 1808, p. 250, 2.

It must, I apprehend, be sufficiently obvious, that upon this plan the respiration was not carried on in the natural manner, and the observations of the experimentalists, although intended to convey the contrary impression, prove that this was the case. They give us the result of ten experiments, the longest of which lasted eleven minutes, and in each of which between 3 and 4000 cubic inches of atmospheric air were respired. They remark that "the operator was scarcely fatigued, and his pulse not raised more than about one beat in a minute; the respirations, however, were deeper and fewer than natural, amounting only to about 58 in 11 minutes, whereas, from repeated observations at different and distant times, he makes 19 in a minute."⁸ I conceive it will be thought that a mode of respiring, which could produce such considerable effects in the short period of 11 minutes, must have been very far from exhibiting the state of the lungs in their natural action.⁹ So far as respects the present question, the absolute amount of oxygen consumed in a given time, it was found that air, after having been once respired, contained only 12.5 per cent. of oxygen, 8.5 per cent. having been consumed and its place supplied by an equal bulk of carbonic acid;¹ hence

⁸ Phil. Trans. for 1808, p. 253.

⁹ As a proof that the mechanical action of the lungs was not performed in a perfectly natural state, I may remark that the quantity of air taken in at a single respiration was no more than $16\frac{1}{2}$ cubic inches, p. 256.

¹ P. 255, 279. This remark only applies to the respiration

we deduce that the average quantity in 24 hours is, under ordinary circumstances, 39534 cubic feet,² equal to 13343 grs. It may appear somewhat remarkable, that although according to the method in which these experiments were performed, the lungs were more completely emptied than in natural respiration, and therefore that the change produced in the air should have been proportionally greater, yet the quantity indicated is less than in the experiments either of Menzies, Lavoisier, or Davy.³

Since the experiments of Messrs. Allen and Pepys, of atmospheric air under ordinary circumstances; when the same air was respired as frequently as possible, until symptoms of suffocation were produced, the quantity of oxygen absorbed is 10 per cent. p. 262.

² Ibid. p. 265, 6.

³ Consumption of oxygen in 24 hours,

According to Menzies	51840 cubic inches, or 17496 grs.
Lavoisier	46048 , or 15541 grs.
Davy	45504 , or 15337 grs.
Allen and Pepys	39534 , or 13343 grs.

But this apparent discrepancy will probably be removed by the consideration, that when these philosophers performed their experiments, it was generally supposed that the atmosphere consisted of .27 oxygen and .73 nitrogen, and as they would no doubt employ the same method of analysis in all cases, we must diminish the proportion of oxygen in all the steps of the calculation. The 27 per cent. of oxygen before respiration, which was supposed to be diminished to 22.5, leaving a deficiency of 4.5, will therefore become 21 before, and 17.4 after respiration, leaving a deficiency of 3.6 only; if we apply this scale of proportion to Sir H. Davy's estimate, it will reduce it from 15337 grs. to 12272 grs., leaving, as might be expected, a superiority in quantity to Messrs. Allen and Pepys's experiments; and the same remark of course applies to the others.

the chemical effects of respiration have been very minutely examined by Dr. Edwards, and he has given us the result of his investigations in a treatise of uncommon merit, whether we regard the decisive nature of the experiments, the clearness and simplicity with which they are narrated, or the extensive information which is displayed on all points connected with the animal œconomy.⁴ Among the most valuable facts of Dr. Edwards's essay is the conclusion which we are led to form respecting the difference in the effect of the respiration of the different animals, and of the same animal under different circumstances. This, while it encreases the difficulty of ascertaining the absolute amount of the changes that are produced in the air, shows us that many of the discrepancies which had been noticed between the results of former experiments, depend not so much upon any inaccuracy in the process, as upon an actual difference in the effect produced, of which the experimentalists were not aware. Although we shall have occasion, in numerous instances, to profit by Dr. Edwards's labours, he does not give us any information respecting the immediate subject of our present inquiry, the absolute quantity of oxygen consumed by a man in a given time.⁵

⁴ De l'Influence des Agens physiques sur la Vie.

⁵ Thenard informs us, in a general way, and without specifying any particular authority, that air, after being once respired, contains from 18 to 19 per cent. of oxygen, a quantity which is greater than what has been assigned by any of the experimentalists who have minutely attended to the subject; *Chimie*, t. iii. p. 666.

With respect to the question which was alluded to above, what proportion of oxygen must there be in air, in order to render it fit for the support of life, we have no facts that can lead us to any very decisive conclusion. As a general principle we find that animals enclosed in a given portion of air die long before the consumption of the whole of the oxygen,⁶ and that the fatal effect, in this case, immediately depends not upon the absence of oxygen, but upon the presence of the carbonic acid which is substituted in its place. The only experiment that I have met with, on which we can depend, as giving us any accurate information on this point, is one of Lavoisier's, in which he found, that when the carbonic acid was carefully removed by caustic potash, as fast as it was formed, a guinea pig could live, without any apparent inconvenience, in air that contained only 6.66 per cent. of oxygen, and even when the proportion was still farther diminished, the only obvious effect produced upon the animal was a degree of drowsiness.⁷ As

⁶ From this remark we must except many of the lower tribes of animals; Vauquelin found that certain species of limax and helix have the power of completely deoxidizing the air in which they are confined; *Ann. Chim.* t. xii. p. 278. et seq. Spallanzani also obtained the same results with various kinds of worms; *Mem. sur la Respiration*, p. 62.

⁷ *Mem. Acad. Scien.* pour 1789, p. 573. Messrs. Allen and Pepys also witnessed the same effect in a similar kind of experiment, in which a guinea pig was inclosed in a portion of air consisting of a mixture of oxygen and hydrogen; *Phil. Trans.* for 1809, p. 424, 428.

the temperature of the guinea pig, and structure of its lungs, is the same with that of man, it may be presumed that, under the same circumstances, human life might be supported by air of similar composition; but this, we must bear in mind, could probably only be the case under the most favourable circumstances, where every extraordinary source of expenditure was guarded against, or did not exist.⁸

The next point which we proposed to examine is the quantity of carbonic acid produced by respiration. The fact of its presence in air that has been respired, I have already mentioned, as one of the most interesting of the discoveries of Black, but it does not appear that he made any attempt to ascertain its quantity. Lavoisier, in his first memoir on respira-

⁸ Halley, in his proposal for improving the construction of the diving bell, *Phil. Trans.* v. xxix. p. 492. et seq. observes that a gallon of air will become unfit for respiration in a little more than a minute. Lavoisier, *Ann. Chim.* t. v. p. 261, informs us that a man cannot live more than an hour in 5 cubic feet of air. Sir G. Blane gives us an important practical observation on this subject, which is deduced from very extensive observation; that in calculating for the arrangements of a hospital, each individual should be allowed a space of 600 cubic feet, below which it will be found impossible to maintain the requisite purity of the air; *Med. Chir. Trans.* v. iv. p. 115. Dr. Edwards has given us the result of his experiments on the extreme limits of the rarefaction of the air, when it appears incapable of supporting life for any perceptible length of time; this for birds he found to be a pressure of a little more than 5 inches, and for guinea pigs of a little more than 3 $\frac{1}{2}$; *De l'Influence &c.* p. 495.

tion, informs us that the air in which an animal had expired contains $\frac{1}{5}$ of its bulk of carbonic acid,² while in the experiments on the guinea pig which was confined in pure oxygen, it appeared to constitute about $\frac{1}{5}$ of the total bulk of the air, when it was reduced to a state in which it was no longer fit for the support of life.¹ These experiments, however, do not give us any information concerning the quantity which is produced under ordinary circumstances.

The solution of this problem appears to have been first attempted by Jurine; he endeavoured to ascertain what proportion of the air that is emitted from the lungs in natural respiration consists of carbonic acid, and this he estimates at about $\frac{1}{10}$ or $\frac{1}{11}$,² a quantity very much beyond the truth. The same kind of calculation was afterwards made by Menzies, although with a very different result, for he conceived that the quantity of carbonic acid in air which had been once respired is no more than $\frac{1}{20}$ of its bulk; from the estimate which he had formed of the total quantity of

² Mem. Acad. pour 1777, p. 189. Crawford informs us that when an animal is confined over water about $\frac{1}{5}$ of the air is absorbed, which must have been carbonic acid; and that the same diminution takes place over mercury if potash be introduced; On Animal Heat, p. 146, 7.

¹ Ann. Chim. t. v. p. 261. et seq.

² Encyc. Meth., Art. Medecine, t. i. p. 494, 5. Jurine's experiments were published in 1787, and said to have been lately performed. Menzies's thesis was published in 1790; it is, however, extremely probable that the account of Jurine's experiments, which, I believe, first appeared in the Encyc. Meth., had not reached this country when Menzies was engaged in his investigations.

air respired in 24 hours, he deduced the amount of carbonic acid formed to be 51840 cubic inches,³ equal to 24105·6 grs.

The quantity of carbonic acid produced in the lungs was one of the points to which Lavoisier and Seguin particularly directed their attention in the experiments which they performed in conjunction; but, notwithstanding the nature of the apparatus, and the minute attention which they appear to have paid to every circumstance which might ensure accuracy, the estimates, in the different sets of experiments, differ considerably from each. In the first set, published in the memoirs of the Academy of sciences for the year 1789, the average quantity of carbonic acid in 24 hours is stated to be 17720·89 grs.,⁴ in the following year, the quantity is diminished by more than $\frac{1}{4}$, to 8450·24 grs.,⁵ and according to La Place's account, it was reduced still lower in Lavoisier's last experiment, to 7550·4 grs.⁶ Sir H.

³ Essay, p. 50.

⁴ Mem. Acad. Scien. pour 1789, p. 577. In referring to the memoirs of the Academy of Sciences, it is necessary to observe that the dates prefixed to the volumes do not correspond to the actual date of publication, as in the present instance, where the volume said to be for 1790, was not published until 1793. In tracing the progress of experimental research, were we not to pay attention to this circumstance, we might be misled as to the respective claims of contemporary writers to the priority of discovery. On this account, although we cannot suppose that any deception was intended, yet the plan of publication adopted by the Academy is objectionable.

⁵ Mem. Acad. Scien. pour 1790, p. 609.

⁶ Suppl. to Encyc. Brit. v. ii. p. 549. No explanation is of-

Davy, on the contrary, estimates the amount of carbonic acid in 24 hours at nearly the quantity which was announced by Lavoisier and Seguin in their first experiment, 17811·36 grs.,⁷ and Messrs. Allen and Pepys very nearly coincide with him in this estimate.⁸

I have already stated, that the absolute quantity of oxygen consumed, and of carbonic acid generated by respiration, is materially influenced by various causes which affect the state of the constitution and

fered by Lavoisier of the cause of these different results, but they may perhaps be reconciled by some circumstances which will be mentioned hereafter.

⁷ *Researches*, p. 434. It is stated that in natural respiration 26·6 cubic inches of carbonic acid are formed in a minute, which will be 38304 inches in 24 hours. Mr. Dalton estimates the carbonic acid produced in 24 hours at 2·8 lbs., or 16128 grs. *Manch. Mem.* 2d. ser. v. ii. p. 27.

⁸ *Phil. Trans.* for 1808, p. 256. In their 11th experiment, which they regard as the one that is the most to be depended upon, they found 26·55 cubic inches produced per minute, or 38232 in 24 hours. They always found that air which had been once respired contained about 8·5 per cent. of carbonic acid; p. 255; and it is remarkable that whether the respiration proceeded at a quicker or slower rate, the proportion of carbonic acid remained the same; p. 257. When the same air was respired for a number of times, the maximum of carbonic acid was 10 per cent.; p. 262. Dr. Prout has given us a valuable synopsis of the results of the different experiments which have been performed for the purpose of ascertaining the quantity of carbonic acid produced; it appears to vary from 10 to 3·45 per cent. of the air inspired; *Ann. Phil.* v. ii. p. 333; but before we can draw any correct deduction from them, it would be necessary to know the bulk of a single inspiration, and the number of inspirations performed in a given time, in each of the cases.

the functions, and it will now be necessary for us to attend to them a little more minutely. Priestley, in an early stage of his experiments, noticed the different effect which different animals of the same species produced upon air, principally as connected with their age or apparent vigour.⁹ Crawford extended his observations to temperature, and showed by actual experiment, that the quantity of air which an animal deoxidates in a given time, is less in proportion to the elevation of the temperature.¹ Jurine appears to have performed a number of experiments on this subject, and to have ascertained that different states of the constitution, in the same individual, influence the chemical change produced upon the air; that whatever quickens the circulation, as the process of digestion, exercise, or the hot stage of fever, increases the quantity of carbonic acid, while, on the contrary, it is diminished by the cold stage of fever or by bleeding.² The experiments performed by Lavoisier, in conjunction with Seguin, confirmed the results of Jurine, and afford much important information respecting the absolute amount of the effect produced under the various circumstances of temperature, exercise, and the process of digestion. As a standard to which the rest of the experiments might be referred, they state that a man at rest, with the stomach empty, and at the temperature of 82° Fah., in the space of an hour, consumes 1210

⁹ Experiments on Air, (1st series) v. i. p. 72. et alibi.

¹ On Animal Heat, p. 311. . 5; 387, 8.

² Encyc. Meth. Art. "Medecine," t. i. p. 494.

French cubic inches of oxygen. If the temperature be lowered to 57° , he will consume 1344 inches; during the process of digestion the quantity will be increased to between 18 and 1900 inches; by violent exercise, the stomach remaining empty, it was farther augmented to 3200 inches, and after taking food it was still farther augmented to 4600 inches.³

A series of very curious experiments, which bear upon this point, was performed by Dr. Prout in the years 1813 and 1814. He discovered the remarkable fact, that the amount of carbonic acid discharged from the lungs appeared to be influenced by the hour of the day, and that this occurred in a very uniform and regular manner. The greatest quantity of carbonic acid discharged from the lungs in a given time was about noon, or more generally, between 11 a. m. and 1 p. m.; from this hour the production of carbonic acid diminishes until it has reached its minimum quantity, at about $8\frac{1}{4}$ in the evening; it remains in the same state until about $3\frac{1}{2}$ in the morning, when it suddenly begins to increase.⁴ The maximum quantity he found to be 4.1 per cent., and the

³ Mem. Acad. Scien. pour 1789, p. 575. It is impossible to avoid noticing and regretting, that Lavoisier, in this paper, has made no mention of the experiments of Crawford, although they were published 14 years previous to the date of his own memoir.

⁴ Can these curious results of Dr. Prout's be explained by the observations that have been made by Dr. Edwards on the transpiration, which he found to be increased during sleep, and also to be greater in the forenoon than towards the evening; De l'Influence &c. p. 316, 321, &c.

minimum 3·3 per cent. of the oxygen inspired; the mean quantity given off in 24 hours is stated to be 3·45 per cent.⁵

Another singular circumstance noticed by Dr. Prout is, that whenever, from any cause, the production of carbonic acid has been either increased or diminished above and below the usual maximum or minimum quantity, it will be found to be inversely diminished or increased in an equal proportion during a subsequent diurnal period. We have the results of a number of experiments, placed in the tabular form, which seem fully to establish the fact; and by comparing the circumstances under which the experiments were made, however difficult it may be to conceive of the mode of operation, it would appear that there is no other assignable cause to which

⁵ Ann. Phil. v. ii. p. 330; and v. iv. p. 331. 4. If we assume the quantity of air respired in the diurnal period to be 1152000 cubic inches, it will give us 39744 inches of carbonic acid produced, an estimate which approaches very nearly to that of Messrs. Allen and Pepys, and to the corrected estimates of Menzies, Lavoisier, and Sir H. Davy. We must remark here upon the different proportion of carbonic acid in the experiments of Messrs. Allen and Pepys, and in those of Dr. Prout, the first being 8·5 per cent., the latter only 3·45. Yet the total quantity of carbonic acid produced is very nearly the same; this depends probably upon the respirations in the former experiments being considerably less frequent, and therefore, we may presume, the inspired air would be more completely mixed with the contents of the lungs, and also from the bulk of a single inspiration being so small in the individual upon whom they were performed; these circumstances may be partly explained by the nature of the apparatus which they employed.

the difference can be attributed.⁶ He afterwards examined into the effect of other circumstances upon the production of carbonic acid, and we learn that exercise, if long continued and violent, always diminishes the quantity of carbonic acid produced; that fasting has the same effect; also alcohol taken into the stomach under any form,⁷ probably also sleep, and certainly the depressing passions, or even strong mental emotions of any kind, while no perceptible effect could be ascribed to a change of temperature. It appears, indeed, that whilst almost every variation in the system decreases the quantity of carbonic acid, there are very few circumstances which were found to increase it, and those only in a slight degree.⁸

About the same time that Dr. Prout was engaged in his inquiry, Dr. Fyfe entered upon a set of experiments respecting the effects which certain substances taken into the stomach produce upon the generation of carbonic acid: the following are the most important of his results. He fixes upon 8.5 per cent. as the quantity discharged from his lungs under ordinary circumstances;⁹ he found that it was much

⁶ Ann. Phil. v. ii. p. 330, 338. .0.

⁷ This appears to be the fair influence from Beddoes's experiments, although his hypothesis inclined him to the contrary conclusion. The experiments generally tend to show that muscular action promotes the consumption of oxygen; On Fact. Airs, part 1. § 7. p. 23. .26.

⁸ Ann. Phil. v. ii. p. 335. .7; also v. xiii. p. 269, 0.

⁹ This quantity agrees exactly with the estimate of Messrs. Allen and Pepys, while it very far exceeds that of Dr. Prout;

diminished by wine, that it was still farther reduced to nearly half by vegetable diet, and to nearly one-third by a course of mercury.¹ The apparatus which Dr. Fyfe employed would not allow of his observing the diurnal variations; and to the same cause we may probably attribute the large quantity of carbonic acid which he supposed to be produced in natural respiration, as it would be necessary for him to use a considerable effort in expelling the contents of the lungs; and we may suppose that he might be induced to empty them more completely, and would therefore discharge a portion of air in a more deoxidated state.

Upon comparing the experiments of Dr. Prout and Dr. Fyfe, which are the more interesting, as they agree in their most important results, although made without consent or co-operation, we must remark that the causes which were found to diminish the production of carbonic acid, are some of them the very same, which, according to Jurine and Lavoisier, produce the directly contrary effect, such as exercise, and the process of digestion, while there are others, as increased temperature, which, according to Crawford and Lavoisier, appeared to produce so powerful an influence upon the respiration, but which, from Dr.

but we cannot, from this datum alone, ascertain the total amount of carbonic acid formed in 24 hours, unless we knew the bulk of each inspiration, and the number of respirations, in a given time.

¹ Ann. Phil. v. iv. p. 334. I take my account from Dr. Prout's abstract, not having been able to procure Dr. Fyfe's work.

Prout's experiments, would seem to be totally ineffective.² It is difficult to conceive how Lavoisier and Seguin could have been mistaken in their results; and it is impossible to impeach the accuracy of Dr. Prout, so that we must consider these discrepancies as among those difficulties which we occasionally meet with, and are unable to explain, but which we may hope will be elucidated by future investigations.³

The researches of Dr. Edwards were directed, among other objects, to the examination of the influence of certain circumstances, both external and internal, upon the respiration, especially as manifested by the production of carbonic acid, of which the most important were the effects of the different periods of life and of the seasons. With respect to the first of these points, he remarks, that from the apparent activity of the system in youth, it had been generally supposed, that all the functions must partake of this energy, and that the respiration would afford the additional supply of air which would seem

² Still further to increase the difficulty of arriving at any certain conclusion upon this point, we are informed by Spallanzani, that his experiments led him to conclude that the quantity of oxygen consumed is in the direct ratio of the temperature; *Mem. sur Respiration*, p. 89, 185.

³ We have some interesting experiments by Nysten, on the generation of carbonic acid in the lungs, as affected by different states of disease; the general result is, that, in obstructions of the lungs, the quantity is obviously diminished, while, on the contrary, it is increased in the state of acute fever; *Recherches*, p. 187. et seq.

to be required for the growth and development of the body. But upon making the experiment, the fact was found to be otherwise. It appeared, that with respect both to birds and to different species of the mammalia, the consumption of oxygen and the production of carbonic acid is less in the early periods of life; and it is probable that there is a progressive increase in these operations until the animal arrives at its mature state.⁴

In giving an account of the effect of the different seasons upon the respiration, Dr. Edwards observes that there are several ways in which the air may be supposed to have its physical properties altered, so as to influence its action upon the lungs; of these the most obvious is its temperature, and, as connected with this, its density and its pressure. It appears to be pretty well established that the increase of temperature, and the consequent diminution of the density of the air, tend to diminish its consumption; although it may be somewhat doubtful what proportion of the effect is to be ascribed respectively to each of these changes. It still, however, remained to be examined whether the variation of the seasons produced any effect upon the respiration, as a vital

⁴ De l'Influence &c. par. 3. c. 5; tab. 51 and 52, p. 633, 4. This circumstance was noticed by Boyle; he placed a kitten, a day old, in his air-pump, and found that it "continued three times longer in the exhausted receiver than other animals of the same bigness would probably have done," Works, v. iii. p. 360. Morozzo had found, in his experiments, that young animals lived longer in a given quantity of oxygen than adult animals of the same species; Journ. Phys. t. xxv. p. 103.

function, independent of the physical state of the air; and this, upon making the experiment, was actually found to be the case. He confined birds in a limited quantity of air, under similar circumstances, at different seasons of the year, when it was found that, in the winter, the lungs possessed a greater capacity for decomposing the air than in the corresponding summer months; an effect which appeared to be brought about by the long continued action of a low temperature upon the constitution.⁵

There is a circumstance respecting the two effects of respiration, which we have been contemplating, the consumption of oxygen and the production of carbonic acid, which we must examine in this place, whether they always take place to the same extent and are proportional to each other. As the question involves many important theoretical conclusions, it has been made a particular object of attention, yet there appears to be still some difficulty in arriving at an accurate conclusion concerning it. In most of the earlier experiments the quantity of oxygen consumed appeared to be greater than that of the carbonic acid produced,⁶ although the exact amount of this dif-

⁵ De l'Influence &c. par. 3. ch. 6; tab. 53 and 54, p. 635, 6.

⁶ It is worthy of observation, that this was the opinion of Priestley, on Air, v. iii. p. 378, 9; and it may be mentioned as one among many instances, where, although the nature of the apparatus which he employed, and the mode in which many of his experiments were performed, seemed but little calculated to obtain very correct results, yet the number of his experiments, and the ingenuity which he displayed in varying them, and comparing them with each other, had the effect of more

ference varied considerably in the different experiments. Lavoisier and Seguin supposed the oxygen consumed in 24 hours to be 15661.66 grs., while the oxygen necessary for the formation of the carbonic acid produced was no more than 12924 grs., or in the proportion of about 100 to 81.5. These numbers coincide almost exactly with what may be deduced from Sir H. Davy's experiments, the oxygen consumed than supplying the deficiency. In Lavoisier's earlier experiments he does not appear to have noticed this disproportion between the quantity of oxygen consumed and of carbonic acid produced; but he afterwards observed it, and ascertained its exact amount to be in the proportion of 229.5 to 54.75, or nearly 4. This surplus quantity of oxygen he supposed to unite with hydrogen abstracted from the blood, and to form water; *Mem. Soc. Roy. Med. pour 1782*, 3, p. 574. There seems to have been some confusion in the references that have been made to the papers of Lavoisier on Respiration, in consequence of the want of correspondence between the time when the papers were read, when they were actually published, and the year inserted in the title of the volume as already mentioned, p. 122. Lavoisier and Laplace published a joint essay on Heat, one section of which is on Respiration and Animal Temperature, in the *Mem. of the Academy of Sciences* for the year 1780; the essay was, however, not read until June 1783, and the volume not printed until 1784. The paper of Lavoisier's referred to above, entitled, "*Sur les alterations qui arrivent à l'air dans plusieurs circonstances où se trouvent les hommes réunis en société,*" was published in the *Mem. of the Royal Medical Society* for 1782 and 3; it was not read until February 1785, nor printed until 1787. This 2d paper appears to be referred to by some writers as the memoir of 1783, while others speak of the former under that designation, the one adhering to the nominal date of the volume, the other to the actual time when it was read.

being 15337 grs., while the carbonic acid produced was 17811.36 grs., which would contain 12824.18 grs. of oxygen, giving a proportion of 100 to 81.66.

But although these experiments appear to prove so decisively that there is a surplus quantity of oxygen consumed, more than is required for the production of the carbonic acid, and although they agree so nearly in the amount of this surplus, yet the conclusion which we might have been induced to draw from them appeared to be shaken, if not overthrown, by the experiments of Messrs. Allen and Pepys, whose researches, conducted as they were with such minute accuracy, were equally convincing in showing, that the oxygen which disappears is exactly replaced by an equal volume of carbonic acid, and that therefore the whole of it must have been employed in the formation of this acid.⁷

⁷ Phil. Trans. for 1808. p. 279. In the experiments of Dr. Prout it is always taken for granted that the oxygen consumed is exactly replaced by an equal volume of carbonic acid; Ann. Phil. v. ii. p. 330. et alibi. The same conclusion is also formed by Mr. Ellis, after a very full and candid examination of all that had been done on this subject by preceding physiologists; Inquiry, p. 116. et seq. Dr. Henry also regards the experiments of Messrs. Allen and Pepys sufficiently decisive to prove that the amount of oxygen consumed exactly coincides with the carbonic acid produced; Elements, v. ii. p. 403. M. Magendie expresses himself very decisively upon the subject; "Quant au volume de l'oxygène en *déficit*, comparé au volume de l'acide carbonique expiré, je dois dire que toutes nos expériences, sans en excepter aucune, sont entièrement d'accord avec celles des chimistes Anglais: nous avons constamment vu l'oxygène disparaître pendant l'acte respiratoire représenté exactement par l'acide carbonique de l'air expiré;" Sur la Trans. Pulm. p. 9.

The state of uncertainty in which we were thus left, the judgment being, as it were, almost equally poised between such high authorities, has been most happily removed by Dr. Edwards, who investigated this point, among others to which he directed his attention; and has shown by a train of remarkably well devised experiments, that the discordancies did not depend so much upon the inaccuracy of the operator, as upon an essential difference in the results. He begins by remarking that experiments of this kind are better performed on small animals, which can be immersed in a large quantity of air, than on man, where the air must be renewed at each respiration.⁸ When experiments were conducted in this manner upon various warm-blooded, vertebrated animals, which, with respect to their respiration, possess a close analogy with man, it has been found that there is generally a diminution of the bulk of the air, but that the quantity of the diminution varied much in the different experiments, and that on some occasions it did not exist.

As it is ascertained that when oxygen is converted into carbonic acid, the bulk of the gas is not changed, the diminution in these cases is presumed to depend upon the absorption of oxygen, or to the quantity of it which is consumed being greater than what is necessary for the production of the carbonic acid. In three experiments upon young dogs, the whole quantity of air employed was 91.542 cubic inches; the process was carried on over mercury, and lasted for

⁸ De l'Influence &c. note to p. 408.

five hours: the average results were that the gas was diminished by 5·675 cubic inches, or about $\frac{1}{16}$ of its volume, and that 10·9 cubic inches of carbonic acid were produced.⁹ Here, therefore, the total quantity of oxygen consumed is 16·575, in proportion to that employed in the formation of the carbonic acid as 16·575 is to 10·9, or as 100 to 60·66. In another series of 10 experiments, in which yellow-hammers were employed, and remained each of them for 15 minutes in 100·6 cubic inches of air, the total consumption of oxygen was upon the average 4·437 inches, while the carbonic acid produced was 3·65, or in the proportion of 100 to 82·26.¹ Dr. Edwards's general conclusion is, that the proportion of oxygen consumed to that employed in the production of carbonic acid, varies from more than one-third of the volume of carbonic acid to almost nothing; that the variation depends upon the species of the animal employed, upon its age, or some peculiarity in its constitution, and also that it varies considerably in the same individual at different times.² The general fact, therefore, of the surplus quantity of oxygen, in a great majority of cases, is abundantly proved, while we may fairly conclude that the source of error in the experiments of Messrs. Allen and Pepys, depended upon the difficulty of bringing the lungs into the same state of distention at the beginning and the end of the experiment, and upon the results

⁹ *De l'Influence &c.* p. 410. et seq. ¹ *Ibid.* p. 415.

² *Ibid.* p. 418.

being complicated by the gas originally contained in the lungs.³

³ Mr. Ellis, in his "Farther Inquiries," has offered many valuable observations on the mechanical part of Messrs. Allen and Pepys's experiments, p. 280. et seq.

Nysten and Spallanzani, from their experiments, the former on the human subject, *Recherches*, p. 214, the latter on various species of the mollusca, *Memoires*, p. 66. et alibi, come to the same conclusion, respecting the want of correspondence between the oxygen and carbonic acid. Legallois also found this to be the case in his experiments, and although they were performed under particular circumstances, and for a specific object, yet they are generally applicable so far as this question is concerned; *Ann. de Chim. et Physique*, t. iv. p. 115. Dr. Thomson, *Chem.* vol. iv. p. 619, and Mr. Dalton, *Manch. Mem.* vol. ii. 2d ser. p. 25, likewise obtained a surplus quantity of oxygen, although, from certain considerations, they were induced to ascribe this difference to incidental circumstances, not essentially connected with respiration; see Dalton, p. 86.

The specific gravity of the serum of arterial blood has been generally found to be less than that of venous; in the experiments of Dr. Davy, the proportion was as 1047 to 1050; *Phil. Trans.* for 1814, p. 591. It has been supposed that, as during the change from the venous to the arterial state, there is an absorption of oxygen and a discharge of carbonic acid, a point which will be more fully investigated hereafter; we might conclude from this change of the specific gravity, that the quantity of gaseous matter absorbed is greater than that discharged. This observation proceeds upon the idea, which is probably correct, that the water which is discharged from the lungs does not immediately proceed from the blood in the pulmonary vessels, but that it is the result of secretion. But it also takes for granted, that the blood loses nothing by serous transudation; for if we conceive that this process takes place as the blood passes through the lungs, it accounts for the difference in the specific gravity of the two kinds of serum. It may be further

The third point which I proposed to examine, whether the bulk of the air be diminished by respiration, is essentially connected with the question which has been discussed above, and indeed almost resolves itself into the same inquiry. All the earlier physiologists supposed the diminution to take place, and they accounted for it upon the idea, that the air had lost part of its elasticity, or, as they termed it, its spring.⁴ Mayow appears to have been the first who attempted to ascertain the exact amount of the diminution; he estimated it at $\frac{1}{13}$,⁵ while Hales, in different experiments, found it to vary from $\frac{1}{13}$ to $\frac{1}{10}$.⁶ But all these statements are much over-rated; a circumstance that depends, in part at least, upon the air of expiration being passed through, and confined over water, which would necessarily absorb a part of the carbonic acid.⁷ Lavoisier, Goodwyn, and Sir H. Davy, however, in their more correct experiments, although they found the diminution to be much less, did not fail to recognize it. Lavoisier, in

remarked upon this subject, that the specific gravity of the serum of different individuals differs at least as much as the difference indicated by Dr. Davy, between the arterial and venous blood; See *Med. Chir. Tr.* v. ii. 170, 363.

⁴ Boyle informs us, that in one experiment, in which a mouse was confined in a portion of air over mercury, the volume of the air was not diminished; *Works*, v. iii. p. 380.

⁵ *Tract.* p. 105. ⁶ *Stat. Essays*, v. ii. p. 238, 320.

⁷ Crawford found that when the air was confined over water, $\frac{1}{10}$ th of the whole was absorbed; on *Animal Heat*, p. 146.

his Memoir of 1777, fixes the amount at $\frac{1}{60}$.⁸ Goodwyn obtained the same result,⁹ Sir H. Davy found that air, which had once passed through the lungs, as is the case in ordinary respiration, suffered a diminution which varied, in his different experiments, from $\frac{1}{60}$ to $\frac{1}{100}$; ¹ Berthollet always found a diminution, although somewhat less in quantity,² and the same result was obtained by Jurine³ and by Spallanzani.⁴ It might be supposed that these results afforded sufficient proof of the general fact of the diminution of the air by respiration, yet we have here, as on the former occasion, a great diversity of opinion. Crawford

⁸ In the experiments where the guinea-pig was confined in oxygen, the diminution was, in one case, $\frac{1}{15}$, and in the other $\frac{1}{6}$ of the volume of the air, the greater diminution in these experiments probably depending upon the increased consumption of oxygen, in consequence of the greater purity of the air employed; Mem. Acad. Scien. pour 1780. p. 401; Ann. Chim. t. v. p. 261; Mem. Soc. Roy. Méd. pour 1782, 3. p. 572.

⁹ Connexion of Life &c. p. 51.

¹ Researches, p. 431.. 3.

² Mem. Soc. d'Accueil, t. ii. p. 454.. 463.

³ Mem. Soc. Roy. Méd. t. x. p. 25.

⁴ Mem. sur la Respir. p. 102. Cuvier also states the fact of the diminution of the volume of air, and fixes it at $\frac{1}{60}$; it does not, however, appear, that he himself performed any experiments on this subject; Leçons d'Anatomie Comp. t. iv. p. 303. Dr. Thomson also found a diminution in the volume of the air, but it varied so much in his different experiments, that he was disposed to ascribe it to some accidental cause; Chem. v. iv. p. 617. Dr. Henry also agrees in the general fact; Elements, vol. i. p. 293.

expressly states that he could not observe any diminution of the volume of the air, when the process was conducted over mercury;⁵ it is not adverted to by Lavoisier and Seguin in their conjoined experiments, while Messrs. Allen and Pepys, in their elaborate researches, although they generally found a slight diminution, attributed it to some accidental circumstance connected with the management of the apparatus, or the nature of the process, and concluded that the bulk of the air is not essentially affected by respiration.⁶ But the experiments of Dr. Edwards again most fortunately relieve our embarrassment, by showing us that the diminution really takes place in a great majority of cases, although, in such various degrees, that we are not able to reduce it to any fixed amount.⁷

⁵ On Animal Heat, p. 147.

⁶ The average diminution of the ten first experiments, p. 253, is not $\frac{1}{100}$; in the eleventh experiment, which they appear to regard as the most correct, it is about $\frac{1}{100}$, p. 254; but they state that the general average of all their experiments is about 6 parts in 1000, p. 281, or $\frac{1}{167}$. It may be presumed, from various expressions in Dr. Prout's papers, that he did not suppose there was any diminution of the volume of the air; Ann. Phil. vol. ii. p. 330. et alibi. Mr. Ellis likewise concludes that the volume of the air is not diminished, Inquiry, p. 99, 0; and M. Magendie's experiments led him to the same opinion; Mem. sur la Transp. Pulm. p. 7. 9. Mr. Abernethy, on the contrary, supposes that the volume of the air is increased; Essays, p. 147.

⁷ De l'Influence &c. p. 411. et alibi; it follows from the view which Dr. Edwards takes of the action of the lungs, that occasionally the bulk of the air may be increased by respiration, that at other times the bulk may be unaffected, but that in a majority of cases it will be diminished.

The fourth point that we proposed to examine respects the absorption of nitrogen, and on this we shall find as much diversity of opinion as on those that have already passed under our review. Lavoisier's experiments led him to conclude that the nitrogen is entirely passive in respiration, or that it serves no other purpose than to dilute the oxygen;⁸ and Messrs. Allen and Pepys deduced the same conclusion from their experiments.⁹ Priestley, on the contrary, supposed that there was an absorption of nitrogen;¹ but his experiments being performed in an early stage of the pneumatic chemistry, and with a less perfect apparatus, notwithstanding the confidence with which he maintained his opinion, the result was

⁸ Mem. Acad. Scien. pour 1777. p. 193; and he still continued to support this opinion in his later essays; see Mem. pour 1789. p. 574. where he says that he has proved this by very decisive experiments.

⁹ Phil. Trans. for 1808. p. 264. et alibi; and Phil. Trans. for 1809. p. 412..5. Messrs. Allen and Pepys conceive that in natural respiration the nitrogen is not affected, but that when the same portion of air is frequently respired, a quantity of nitrogen is discharged; Phil. Trans. for 1808. p. 263. The same effect was also produced by the respiration of pure oxygen, Phil. Trans. for 1809. p. 404, 415..421, 427. They remark, with justice, that an apparent increase in the portion of nitrogen may depend upon the quantity of it which exists in the lungs before the experiment; they proved, however, by causing an animal to respire a mixture of oxygen and hydrogen, that, in certain cases at least, nitrogen is actually evolved, p. 420..427. Cuvier, Tabl. Elem. p. 46, supposes that the nitrogen is not affected by respiration.

¹ On Air, v. iii. p. 380.

generally attributed to some accidental occurrence. Priestley's conclusion has, however, been powerfully confirmed by subsequent experiments; Sir H. Davy conceived it to be the case in his experiments, and he estimated that 5.2 cubic inches of nitrogen were absorbed per minute,² or about 7488 inches in the 24 hours, a quantity equivalent to 2240 grains. The absorption of a portion of nitrogen is maintained by Cuvier,³ and has been proved by the researches of Dr. Henderson,⁴ and Prof. Pfaff,⁵ each of whom instituted a series of well conducted experiments, which nearly coincided in their results. They both of them indicated a deficiency of nitrogen in the air of expiration, although they differed somewhat in the amount; Dr. Henderson supposing it to be less, and Prof. Pfaff more, than the estimate of Sir H. Davy. There are indeed certain points in which these experiments would appear not to be altogether unexceptionable, but they fully warrant the conclusion which the authors deduce from them with respect to the question now under consideration. It may be observed also that they both of them agree in supposing that the total bulk of the air is diminished by respiration.⁶ To add to the apparent confusion of opinion on this subject, Jurine was induced to conclude, from the result of his experiments, that nitrogen is gene-

² Researches, p. 431.

³ Leçons d'Anat. Comp. t. iv. p. 303.

⁴ Nicholson's Journ. v. vii. p. 40..5.

⁵ Ibid. v. xii. p. 249. et seq.

⁶ Ibid. v. vii. p. 49, 4; and v. xii. p. 251; 2.

rated by respiration; ⁷ and the same result was obtained by Berthollet, ⁸ and Nysten. ⁹ The experiments of Berthollet and Nysten seem to warrant the conclusion that is drawn from them; but with respect to those of Jurine, it may be doubted whether they are equally conclusive, as the nitrogen which he supposed to be generated may, with more probability, be referred to a portion of the residual air of the lungs mixed with the air of expiration.

The experiments which have been referred to above were performed either on man or on some of the warm-blooded vertebrated animals, whose respiration may be conceived to produce similar changes on the air. But we are in possession of many very curious facts respecting the respiration of the cold-blooded animals, which are not to be disregarded in forming our judgment upon this subject. Spallanzani's researches on the respiration of the cold-blooded quadrupeds appear to show very clearly that they absorb nitrogen in respiration; ¹ and the experiments of Humboldt and Provençal, on fishes, place the fact beyond all doubt, so far as these animals are concerned. The quantity of nitrogen varied very considerably in the different experiments, from 20 to as much as 89 per cent., while the relation which it bore to the carbonic acid produced was also variable,

⁷ Encyc. Meth. "Medecine," t. i. p. 493.. 7.

⁸ Mem. d'Arcueil, t. ii. p. 454.. 463.

⁹ Recherches, p. 186, 215; from p. 187 to p. 200 is an account of his experiments.

¹ Mem. sur la Respir. p. 184, 258.

although, for the most part, an increase in the one was attended with an increase in the other.²

The researches of Dr. Edwards on this point have been no less successful than on the other objects to which he directed his attention. By immersing small animals in a large quantity of air, for a limited period, and calculating what effect the air contained in their lungs before and after the experiment would have upon the whole mass on which he operated; he found, that in many instances, there was an evident increase in the quantity of nitrogen, while in others there was a deficiency of it. He observed that the former change took place when the experiments were performed in spring or summer, or when young animals were employed, while the latter occurred during the winter. Hence we have the important fact established, that nitrogen is, according to circumstances, either exhaled or absorbed in respiration; the probability is, that in all cases, both these operations are going forwards, that they are often exactly balanced, so as to show neither excess nor deficiency of nitrogen in the expired air, while in other cases, depending, as it would appear, principally upon temperature, or upon the age of the

² Mem. d'Arcueil, t. ii. p. 359. et seq.; p. 378 consists of a tabular view of the results of the experiments. I shall refer my reader to Mr. Ellis's judicious observations on these experiments, which may lead us to doubt whether we can implicitly rely upon the exact quantity of effect produced; they do not, however, appear to me, in any degree, to invalidate the general conclusion; Farther Inquiries, p. 264. et seq.

animal, either the absorption or the exhalation is in excess, producing a corresponding effect upon the composition of the expired air.³

It now remains for me to offer some remarks upon the aqueous vapour which is contained in the air of expiration. The discharge of water from the lungs was a circumstance which must have been noticed by the most cursory observers, and we shall accordingly find that it was much insisted upon by the earlier physiologists, who indeed regarded it as one of the principal uses of the function of respiration. Sanctorius appears to have been among the first who attempted to estimate the amount of the pulmonary exhalation with any degree of accuracy; he supposed it to be half a pound in the 24 hours; but neither the mode in which he conducted his experiments, nor the reasoning which he employed respecting them, were calculated to produce any correct conclusion.⁴ Hales adopted the method of passing the air that was emitted from the lungs through a flask filled with wood-ashes, and by observing what addition of weight it had acquired, he ascribed this to the moisture which the potash contained in the ashes had imbibed; this he estimated at 9792 grs., or about 20 oz. in the 24 hours.⁵ Menzies received

³ De l'Influence &c. p. 420. et seq.; Tab. 62. 66.

⁴ *Medicina Statica*, by Quincy, Aphor. v. p. 45.

⁵ *Statistical Essays*, v. ii. p. 322. 4. I may observe that Haller has deviated from his usual accuracy in speaking of the estimates that have been formed on this subject. Home states, not quite correctly, that Hales obtained 23 oz. of water in 24 hours;

the air of expiration in an allantoid, and by weighing it before and after the experiment, ascertained what additional weight it had acquired; in this way he calculated, that the quantity of water discharged in 24 hours is equal to 2880 grs. or about 6 oz.⁶ Mr. Abernethy breathed into a glass vessel, adapted for the purpose, and collected 180 grs. in an hour, which will give us 4320 grs. or 9 oz. in 24 hours; but he supposes that the fluid contains a quantity of mucus dissolved in it, the proportion of which he did not ascertain, but which must be deducted from the total amount.⁷

The difficulty which there is in actually collecting the water exhaled from the lungs may probably have induced Lavoisier, in his later and more elaborate experiments, to endeavour to ascertain the quantity by an indirect method. He first determined the quantity of oxygen consumed, and of carbonic acid produced; and as he always supposed that the oxygen which had disappeared was more than sufficient to form the carbonic acid which he obtained, he conceived that the excess of oxygen was employed in uniting with hydrogen that was given off by the lungs, and thus generating water.⁸ The

Med. Facts, p. 238; and Haller, in relation to Home's estimate, says, "ad uncias 23 æstimat Cl. Home," and refers to the above passage in Home's work; *El. Phys.* viii. 5. 40.

⁶ Dissertation, p. 54. ⁷ Essays, p. 141.

⁸ I have already stated that Lavoisier, in his first memoir, does not advert to the aqueous vapour which is exhaled from the lungs; it is in the memoir on the respiration of the guinea-

quantity of oxygen being known, that of the water was easily calculated, but the estimates of the aqueous vapour, which Lavoisier formed in his different sets of experiments, differ very much from

pig in oxygen, that he first advances the hypothesis stated in the text. He notices the excess of oxygen above what is necessary to form the carbonic acid, and remarks, that it must either have been absorbed by the blood, or have combined with hydrogen discharged from the lungs, and have produced water; the latter supposition he conceives to be the most probable; *Mem. Soc. Roy. Med. pour 1782*, 3. p. 574; *Ann. Chim.* t. v. p. 264, 5. He does not very clearly state the grounds of this preference, but it may be inferred that it depended upon his conceiving that more caloric was given off by the formation of the carbonic acid in the lungs, than by the formation of the same quantity of carbonic acid by the combustion of charcoal; and this excess of caloric he imagined might be accounted for by the union of the excess of oxygen with a quantity of hydrogen; See *Mem. Acad. pour 1789*. p. 569. I may remark that Crawford had previously stated, as the result of his experiments, that water, as well as carbonic acid, is generated by respiration; *On Animal Heat*, p. 154, 347, 8. In the memoir for 1789, Lavoisier refers to the memoir of 1780, written in conjunction with La Place, for a proof of the fact here stated, respecting the excess of caloric; but upon examining the latter paper it appears to me to warrant the contrary conclusion; see p. 405 and 407. See the remarks of Magendie, in his *Mem. sur la Transpiration Pulmonaire*, p. 4. 6. This physiologist gives a curious case of an individual who had an opening in the upper part of the trachea, and it appeared that when he breathed through this aperture, scarcely any vapour was mixed with the expired air; he also relates some experiments on animals, which lead to the conclusion, that at least a large portion of the expired vapour proceeds from the membrane lining the mouth and fauces; *Ibid.* p. 137, 5.

each other. In the memoir of 1789 the water is stated to be 337.18 grs., while the carbonic acid is 17720.89 grs. or nearly as 19 to 1000; in the memoir of 1790 the quantity of water was increased to 11188.57 grs. while the carbonic acid was reduced to 8451.24 grs., or as 1323 to 1000; and in the posthumous experiments the water is stated to be 13704 grs., the carbonic acid being 7550.4 grs., or nearly as 1815 to 1000. From these discordant estimates it is impossible to draw any conclusion, except that the method itself is one which cannot afford us any accurate results.

Nor, independent of this circumstance, does it appear to be one on which we ought to rely with any degree of confidence. The position on which the whole reasoning rests, the exhalation of hydrogen from the lungs, has never been attempted to be directly proved; it does not appear to bear any analogy to the other operations of the animal œconomy, nor is there any fact with which I am acquainted that seems to countenance it, while it is impossible not to perceive that there is a much more direct and probable source of the aqueous vapour, in the evaporation of water from the surface of the pulmonary passages, or even in transudation through the membranes investing these parts; but I shall have occasion to revert to this subject when I come to consider the changes produced upon the blood by respiration.²

² Dr. Thomson, by a calculation founded upon the force of vapour in the expired air compared with that in the atmosphere,

Having now examined in succession the various effects which respiration has been supposed to produce upon the air, I shall briefly recapitulate the result of our inquiry. 1. Air which has been respired loses a part of its oxygen; the quantity varies considerably, not only in the different kinds of animals, but in different animals of the same species, and even in the same animal at different times, according to the operation of certain external agents, and of certain states of the constitution and functions. Upon an average we may assume that a man, under ordinary circumstances, consumes about 45000 cubic inches, or nearly 15500 grs. of oxygen in 24 hours. 2. A quantity of carbonic acid is produced, the amount of which varies very much according to circumstances, both external and internal; its quantity depends, to a certain extent, upon the quantity of oxygen consumed, but the two are not in exact proportion to each other; in a great majority of cases the quantity of carbonic acid produced will be found to be less than that of the oxygen consumed, so that there will be a surplus quantity of oxygen more than is necessary for the production of the carbonic acid. In consequence of the variations estimated that he discharged from the lungs nearly 19 oz. in 24 hours; *Chem. v. iv. p. 621, 2.* Mr. Dalton, by a similar process, estimates the quantity at 1.55 lb.; *Manch. Mem. v. ii. 2d ser. p. 29.* So far as we are able to apply to the living body the results of experiments made upon the dead subject, we may suppose, from the statement of Reisseissen, that the arteries of the lungs are peculiarly adapted for exhalation; *Ed. Med. Journ. v. xxi, p. 453. et seq.*

which take place in the amount of the carbonic acid produced, it appears almost impossible to fix upon any number which may indicate the average quantity; but it may be stated to be somewhere about 40000 cubic inches in 24 hours. This will weigh 18600 grs. or nearly 3 lbs., and will contain 5208 grs. of charcoal and 13392 grs. of oxygen, which will be 2100 grs. less than the quantity of oxygen consumed. 3. The volume of the air is diminished by respiration, but this, like the changes mentioned above, varies so much at different times, that it is almost impossible to form any statement of the quantity; perhaps, we

It may be not uninteresting to the student of physiology to remark upon the singular vacillation of opinion that has taken place on this subject. About 20 years ago the doctrine of the absorption of oxygen was very generally embraced, all the facts and analogies appearing to be in its favour. After some time, however, it was almost universally discarded, in a great measure, as it would appear, in consequence of the experiments of Messrs. Allen and Pepys; see Berzelius on Animal Chemistry, p. 30. 2; while, I apprehend, that the more recent investigations of Dr. Edwards, taken in conjunction with the former facts and analogies that were adduced in its favour, will cause us to revert to the conclusion which is stated in the text. I may refer to the amicable controversy that took place on this point between Mr. Ellis and myself; *Ed. Med. Journ.* v. iv. p. 159, 320. I conceive that the opinion which I attempted to defend in my paper has since received, from various quarters, but especially from Dr. Edwards, the most unequivocal support. I may say this with the more propriety, because the experiments of Messrs. Allen and Pepys appeared so favourable to Mr. Ellis's doctrine, that I became a convert to it, and supported it in my lectures on physiology; and in the article "Physiology," in *Dr. Brewster's Encyc.*, written in 1823.

may assume that air, which has been once respired, is diminished by about $\frac{1}{80}$ of its bulk. 4. It appears probable that nitrogen is both absorbed by the lungs, and exhaled from them; but the two processes of absorption and exhalation differ very much, both in their absolute quantity, and in the relation which they bear to each other, so that the proportion of nitrogen in the air is sometimes diminished by respiration, is occasionally increased, and frequently remains without alteration. 5. A quantity of aqueous vapour is discharged from the lungs, mixed with, or diffused through the air of expiration; but we have not sufficient data from which to decide upon its amount, and it is probable that the quantity varies considerably in the different conditions of the system and the different situations in which the body is placed.

§ 4. *The Change produced upon the Blood by Respiration.*

The change which is produced upon the blood by respiration involves an inquiry of a much more difficult solution than that respecting the change in the air, in proportion to the greater difficulty of ascertaining the chemical nature of the ingredients of the blood. Indeed, so complicated is this fluid in its composition, and so peculiar is its constitution, that scarcely any attempts have been made to investigate the effect which respiration produces upon it, by examining the substance itself; all that we are able to accomplish is to deduce this effect from observing

the changes which we find to have taken place in the air, assuming that the blood has been the medium by which they were brought about.²

The extreme vascularity of the lungs, and the great proportion of blood which is sent to them, induced even the earliest physiologists to suppose, that some important effect is produced upon this fluid by respiration: this idea was strongly countenanced by the discovery of Harvey, that every portion of the blood passes through the lungs at each complete circulation; and it was still farther confirmed by the observation, that the change from venous to arterial blood takes place in the capillaries of the lungs, and that the air is essential to it. The opinions that were entertained respecting the nature of this operation, and the manner in which it is effected, were very various, but they may be all reduced to three classes.³ A numerous and learned

² It was a question with the older physiologists, whether there was any essential difference between arterial and venous blood; and it would appear, that those who believed that there was a difference, derived their opinion rather from theory than from actual observation. Haller himself doubts, or rather disbelieves, the difference; *El. Phys.* v. 1. 4, 5. A considerable degree of the uncertainty which prevailed among physiologists, before the time of Harvey and Lower, depended upon their being ignorant of the relation between the systemic and the pulmonic circulation, and of the exact point in the circulation, where the venous was converted into arterial blood. Magendie has given us a useful synopsis of the external characters of the two species of blood in a tabular form; *Physiol.* v. ii. p. 288.

³ An interesting, and, upon the whole, a correct account of the various opinions entertained on the use of respiration is

body of physiologists supposed the effect produced on the blood to be merely mechanical.⁴ Some of them thought that the particles, by the agitation which they must experience in passing through the pulmonary vessels, were more completely comminuted or mixed together, so as to render the mass of an homogeneous consistence. This idea depended upon the supposition, that the velocity of the blood was greater during its passage through the lungs than in the other parts of the circulation; a supposition which Hales conceived to be decisively proved by actual observation,⁵ and which was, at one time, very generally adopted. There were others, however, who thought that the motion of the blood could not be quicker through the lungs, and others again who thought it must even be slower in this part of its course, founding their opinions principally upon certain anatomical considerations, connected with the structure of the heart and its great vessels. We shall probably be induced to coincide in the opinion of Haller, that the average velocity of the blood through the lungs is not greater than through the other

prefixed to Priestley's Essay; *Phil. Trans.* for 1776, p. 226. et seq. or *On Air*, v. iii. p. 350.

⁴ As a specimen of the mechanical method of reasoning upon this subject, the dissertation of Sauvages, on the action of the air upon the blood, which was written about the middle of the last century, may be read with advantage, being the production of a man of extensive information, who may be supposed to have been possessed of all the science of his age. See *Cæuvres Diverses*, t. ii. p. 139. et seq.; see also Pitcairne's *Dissert.* No. 4.

⁵ *Statistical Essays*, v. ii. p. 66.

parts of the body; but that its momentum must be much less, because it has fewer obstacles to its progress, a circumstance which is sufficiently indicated by the comparative weakness of the right ventricle.⁶ Boerhaave and his disciples thought that the blood acquired its peculiar organization in the lungs, but they do not appear to have thought it necessary to inquire in what way the effect was brought about.⁷ The question whether the blood was rarefied or condensed in the lungs was zealously contested by the mechanical physiologists, one party supposing that the addition of a portion of the air must render the blood specifically lighter,⁸ while others conceived that the exhalation of the aqueous vapour, and the contact of the cold air, must increase its specific gravity.⁹

A second class of physiologists, in which we find the illustrious names of Harvey,¹ Boyle,² Hales, and Haller, supposed that the blood, in its passage through the lungs, discharged some noxious matter, which, together with the aqueous vapour, was removed by respiration;³ while a third class, among

⁶ *El. Phys.* viii. 5. 21.

⁷ *Prælect. notæ* ad § 200. t. ii. p. 93; § 210. t. ii. p. 115, 6.

⁸ Baglivi, *Opera*, p. 457.

⁹ See the elaborate dissertation of Helvetius; *Mem. Acad. Scien.* pour 1718, p. 230. et seq.

¹ *De Motu Cordis*, p. 232.

² *Works*, v. i. p. 99. et seq.; v. iii. p. 371. et seq.

³ It may be interesting to observe how far the genius of Vesalius enabled him to ascertain the nature and uses of respiration. "*Postquam vero aër ab hac substantia*" (*pulmonis*)

whom we may rank Lower, Hooke, Mayow, and many of the Italians,⁴ conceived that the air imparted something to the blood, by which it was converted to the arterial state. We shall find that none of these opinions is strictly correct, even in the outline, and when their respective advocates proceeded to give them more in detail, they quickly degenerated into mere fanciful hypotheses.

Soon after Harvey had completed the discovery of the circulation, the difference between the colour of the arterial and the venous blood was clearly pointed out, and Lower ascertained that the change of colour took place in the capillaries of the lungs. Be-

" cordi quodammodo præparatus est, a venalis arteriæ surculis, pulmoni etiam intextis, ex asperæ arteriæ ramis elicitor et in sinistrum cordis ventriculum delatus, tenui admodumque fervido, quem cor inibi continet, sanguini commiscetur. Hujus aëris qualitates, contenti in hoc ventriculo caloris qualitas eventilatur, substantia autem caloris (quæ aëre et spirituosi sanguinis exhalatione constat) istius aëris substantia enutritur. Quod vero velut fuliginosum ex hoc peculiari cordis functione congeritur, rursus per venalem arteriam in pulmonem allegatur; &c." Corp. Hum. Fab. lib. 6. c. 1; t. i. p. 492, 3.

* See Boer. Prælect. § 203 cum notis, et Haller, El. Phys. viii. 5. 12, 3, for an account of the earlier physiologists who adopted this opinion. To the names mentioned by Haller we may add that of Mead, Works, v. ii. p. 42; and of Whytt, Works, p. 31. Robinson, who was a physiologist of considerable acuteness, lays down the following proposition, and endeavours to prove it by experiment. "The life of an animal is supported by acid parts of the air mixing with the blood in the lungs; which parts dissolve and attenuate the blood and preserve its heat; and by both these keep up the motion of the heart;" Prop. 24. p. 187.

fore his time the bright scarlet colour of arterial blood had been ascribed by some to a kind of combustion, which is kept up in the heart, by others to the breaking down of the red particles, or to other causes equally inadequate and equally unfounded. By opening the thorax of a living animal he perceived the exact point in the circulation, where the change of colour takes place, and he proved that it was not in the heart, because it still remains purple when it leaves the right ventricle. He then kept the lungs artificially distended, first, with a regular supply of fresh air, and afterwards with the same portion of air without renewing it, when the result was that, in the first case, the blood underwent the usual change of colour, while in the second it returned to the left side of the heart, still retaining its purple hue. Hence he naturally and correctly concluded, that the alteration of colour is effected by the air, and he still farther enforced his opinion by observing the action of the air on the crassamentum of the blood out of the body, which, so far at least as the colour is concerned, he found to coincide exactly with what takes place in the lungs.⁵ We are

⁵ De Corde, p. 175. . 181. Experiments similar to those of Lower have been so frequently repeated, as scarcely to require any particular reference; as a specimen those of Dr. Philip may be mentioned; Phil. Trans. for 1815. p. 71, 2. ex. 7, 8. The effect of respiration upon the colour of the blood is well illustrated by those cases which are termed Cæruleans, where, in consequence of a mal-conformation of the heart or its appendages, the blood is not duly transmitted through the lungs. See Wm. Hunter's two cases in Med. Obs. and Inq. v. vi. p. 291; Sandi-

now so familiar with the facts mentioned by Lower, and are so well assured of the general correctness of the method by which he accounted for them, that it is impossible not to feel surprise at the little impression which his opinions produced upon his contemporaries. We learn, however, that they were almost entirely disregarded, and so completely was the attention fixed upon the mathematical hypothesis,⁶ and so permanent an influence had it acquired over the minds of physiologists, that even Haller decidedly opposed the doctrine of Lower.⁷

forth, *Obs. Anat. Path.* t. ii. p. 11. et seq.; the same translated with some additional observations in *Beddoes on Calculus*, p. 62; *Abernethy's Essays*, p. 2. p. 158. There is a case of this kind related by Mr. Standert, in *Phil. Trans.* for 1805, p. 228; which deserves notice in consequence of the structure of the heart being exactly similar to that of some of the amphibia; it had only one auricle, and one ventricle.

⁶ It is amusing to observe the air of confidence and self-satisfaction with which Pitcairne opposes his mathematical hypothesis to the experiments of Lower; *Dissert.* p. 69, 0.

⁷ Boerhaave, *Prælect. notæ* ad § 203. t. 2. p. 107; *El. Phys.* vi. 3. 17. The manner in which Haller speaks of Lower is still more worthy of remark than the above observations of Pitcairne, as proceeding from one so much better fitted to form a judgment upon the subject. Speaking of the effect of the air upon the part of the crassamentum which was exposed to it, he adds, "*Hoc vulgare experimentum non a Lowero solum, verum etiam ab Helvetio serio propositum est.*" The remark with which Haller concludes his section on the use of respiration is much more characteristic of his candid and philosophical turn of mind. "*Parum forte satisfactum est multis, neque certe non laude dignissimis viris, qui tanta in respirationis per univ-
sum animalium genus constantia perspecta, nobilius aliquod per*

After a considerable time the doctrine of Lower was revived by Cigna, of Turin, who performed a set of experiments for the purpose of proving that the change in the blood from the purple to the scarlet colour, always depends upon the action of the air; but although they appear sufficient to establish this point, they excited little attention, and Cigna himself, in a subsequent memoir, seems half inclined to desert his former opinion. The opinion of Cigna, as I have already observed, was taken up by Priestley, and confirmed by a series of new and varied experiments, while they led him to the farther discovery of a train of facts, which have served as the basis of all the information that has been since gained upon the subject. The action of the air on the blood, which, as we have seen, had been previously admitted, rather as a plausible conjecture than as a deduction from facts, was now proved by direct experiment. It was found that a piece of purple crassamentum, when introduced into a portion of air, assumed the scarlet colour, while the air experienced the same change as by respiration. Priestley afterwards examined the effect which would be produced on the blood by the constituents of the atmosphere applied

pulmones beneficium vitæ animali accedere suspicantur, quam quidem sunt a nobis exposita munia. Eos viros unice velim mihi non succensere, quod id officium, ut mihi nondum cognitum interim omittam, usquedum quid sit, perspiracior intellexero;" *El. Phys.* viii. 5. 24. He had informed us in the previous section, 23, that he considered the formation of the voice as the principal use of respiration.

separately, as well as by the other gaseous fluids which had been recently discovered. Purple crassamentum was reddened more rapidly by oxygen than by the air of the atmosphere, while the contrary effects were produced by nitrogen, hydrogen, and carbonic acid, the scarlet crassamentum being reduced by these to the purple colour. The conclusions from these experiments are highly important; they show that the alteration of colour which the blood experiences in the lungs depends upon the oxygenous part of the atmosphere, and reciprocally, that the change produced on the air by being received into the lungs depends upon the action of the blood in the pulmonary vessels. In order to render the resemblance between his experiments and the actual state of the lungs more complete, Priestley introduced a piece of moistened bladder between the crassamentum and the air, when he found that the same change was effected as in the former case; he also found that the action of the air upon the blood was not interrupted by the intervention of a stratum of milk or serum, but that water and some other fluids which he tried, prevented the change from taking place. The change which, in these cases, takes place in the air, Priestley supposed to be similar to that produced by combustion, and, according to the hypothesis then generally embraced, it was conceived to consist in the addition of phlogiston; he consequently concluded, that the abstraction of a portion of phlogiston constituted the principal difference be-

tween venous and arterial blood, and that this removal of phlogiston was the chief use of respiration.⁸ I have noticed above the modification which Lavoisier introduced into Priestley's hypothesis, depending upon his more correct views of the nature of what had been styled the phlogistic processes; proceeding upon Black's discovery of carbonic acid in the air of expiration, and his own discovery of the constitution of this acid, as consisting of oxygen and carbon, he concluded that the essential difference between arterial and venous blood consists in the latter containing a larger proportion of carbon. To this deduction from well established facts Lavoisier afterwards added the more doubtful hypothesis of the discharge of hydrogen; and although no direct evidence was adduced in favour of this doctrine, so great was the authority attached to every opinion of Lavoisier's, that it obtained almost universal consent,⁹ and the phlogiston of Priestley was accordingly converted into hydrocarbon.¹ But the discharge of hy-

⁸ On Air, v. iii. p. 362..374; Phil. Trans. for 1776, p. 147.

⁹ See Essay on Respiration, p. 228, for references to various writers, both English and Continental, who embraced the opinion that hydrogen is discharged from the lungs; the list, if necessary, might be extended.

¹ The most complete, and, as we may presume, matured account of Lavoisier's doctrine is contained in the paper written by himself, in conjunction with Seguin, and published in the Mem. Acad. Scien. for 1789, p. 566. et seq. We have also a good abstract of Lavoisier's doctrine, and his successive discoveries given by Fourcroy in his "*Médecine Eclairée*," t. i. p. 56..61. published in 1791. See also Seguin's paper on va-

drogen from the lungs, as it rested upon little more than conjecture, was gradually abandoned, and the former doctrine was again adopted, that the chemical change in the blood consists principally in the separation of a portion of carbon.²

We are, however, under the necessity of modifying, or rather of correcting this conclusion in consequence of the views which have been taken respecting the changes produced on the air; for, besides the conversion of oxygen into carbonic acid, by the abstraction of carbon from the blood, it also appears that a portion of oxygen is, in some way or other, received into the system, and that a mutual interchange of nitrogen is always going forwards between the air and the blood, so that at some times the blood has its proportion of this element absolutely diminished, and at other times increased. With respect to the water which is carried off by the expired air, it is probable that this depends upon evaporation from the surface of the pulmonary cavities; or if any

rious topics respecting heat in *Ann. Chim.* t. v; in this essay he points out the connexion between respiration and animal temperature, p. 259, O, and afterwards gives an account of the nature of the change by which arterial is converted into venous blood; this he supposes is by the addition of hydrogen, and that by the union of this hydrogen in the lungs with oxygen, the blood becomes again arterialized; he remarks that hydrogen, as produced from animal substances, always contains carbon, and that this carbon also unites with oxygen and produces carbonic acid. The production of the carbonic acid is therefore considered as a kind of incidental or secondary effect; p. 262. S.

² *Thenard, Chimie*, c. 3. sect. 2. t. iii. p. 666.

part of it should be secreted from the blood itself, as it passes through the lungs, this must be regarded as only indirectly connected with the process of respiration.

It would appear, therefore, that by far the most important change which the blood experiences, at least so far as quantity of effect is concerned, consists in the removal of a portion of its carbon. Some attempts have been made to prove that venous actually contains more carbon than arterial blood, and the results are said to correspond with the hypothesis,³ but the evidence of its truth must principally rest upon a knowledge of the changes which take place in the air. The air certainly acquires carbon by being brought into proximity with the blood; there is no assignable source whence the carbon can be acquired except the blood; the blood obviously undergoes some change from the action of the air upon it, and the crassamentum, when removed from the vessels, affects the air in the same manner with respiration.

But although the fact be thus established, the manner in which this change is effected is much less easy to comprehend than the nature of the change. Two hypotheses have been formed to account for the operation, each of which is supported by the authority of great names, and by many ingenious arguments, as well as by direct experiment. According to one hypothesis, which may be regarded as that originally proposed by Black, and adopted by Priest-

³ Abildguard, in *Ann. Chim.* t. xxxvi. p. 91. et seq.

ley,⁴ Lavoisier, and Crawford,⁵ the oxygen of the inspired air immediately attracts carbon from the venous blood, the carbonic acid being directly generated by their union. According to the other, the oxygen is absorbed by the blood, is mixed with it, and unites with a portion of its carbon; when the blood, in the course of the circulation, again arrives at the lungs, the carbonic acid that had been formed is discharged, while a fresh portion of oxygen is absorbed. The essential difference between the two hypotheses may be expressed in the following query; are the changes induced by respiration entirely effected in the lungs, or are they brought about in the body at large, the lungs serving merely as the organ by which the substances are absorbed or discharged? The first of these hypotheses has the recommendation of being the most simple, but several objections were urged against it, which gave rise to the more complicated hypothesis that was proposed by La Grange.

⁴ Priestley originally took this view of the subject, but he afterwards thought it more probable that the oxygen is absorbed by the blood; an opinion which he appears to have adopted in consequence of his supposing that the quantity of oxygen which disappears is greater than what is necessary to form the carbonic acid; *Phil. Trans.* for 1790. p. 106 et seq.

⁵ It is not intended by this expression to signify, that, at this period, Black, Priestley, and Crawford, had a correct conception of carbonic acid, as consisting of carbon and oxygen, which was a subsequent discovery of Lavoisier; Black announced the actual formation of carbonic acid, while Craw-

In order to form a judgment of their respective merits, as well as to complete the theory of respiration generally, it is necessary to inquire into the source of the carbon which is removed from the lungs, and to consider the probable effect which would result from its union with oxygen, according as it may take place in the lungs only or in the course of the circulation. The first attempt to explain the mode in which the blood acquires its inflammable matter was made by Crawford. He observes that the particles of which the body is composed have a tendency to change, the old ones are perpetually removed, while fresh matter is continually deposited in their room. This gradual interchange of particles is effected by the capillary vessels; the arterial blood conveys nutritious matter to all parts of the body,

ford and Priestley supposed that an inflammable matter was discharged from the blood, which converted part of the inspired air into carbonic acid; See *Mem. Acad. pour 1775*. p. 520. . 6; also Black's *Lect. by Robison*, v. i. p. 99, where he fully admits of Lavoisier's claim. The successive steps by which we arrived at a correct opinion respecting the constitution of carbonic acid, and the share which Lavoisier had in the discovery, are well pointed out by Mr. Aikin, in the article "Lavoisier," *Gen. Biog.*, v. vi. p. 162. Lavoisier's paper referred to above was first read to the Academy in 1775, read a second time in 1778, and published in the same year. In his memoir of 1777, p. 191. he clearly states the two hypotheses of the absorption of oxygen and of its direct conversion into carbonic acid, and thinks it probable that both the operations may take place, although, as we have seen, he afterwards determined exclusively in favour of the latter opinion. See Dr. Edwards, *De l'Influence &c.* p. 437, 8.

and employs it in repairing the waste that is necessarily going on, while, at the same time that the blood loses its nutritive particles, it receives the effete or putrescent matter, which is now become useless or even noxious to the system; this is carried by the veins to the lungs, and is there discharged, after being united to oxygen.⁶ It is to this change of particles that the difference between arterial and venous blood is ascribed, and it follows, according to this view of the subject, that the matter which is received into the systemic veins contains more carbon than that which is carried off by the arteries, and is employed in the growth and nutrition of the body.

Crawford's hypothesis possesses much ingenuity; it accords with some well established facts, and seems to afford a simple and natural explanation of them, yet, upon a closer inspection, it will be found to be inadmissible. We have no evidence of the existence of any set of vessels or other apparatus, by which the carbon can enter the veins at their capillary extremities, while there is an obvious source of this matter in the chyle which is poured into them, near their termination in the right side of the heart,

⁶ On Animal Heat, p. 150, 1. He brings forward a direct experiment of Hamilton's, in order to prove that blood is venalized by the addition of the basis of hydrogen, p. 149, 0; but the experiment is not of that nature which can enable us to draw any important consequences from it. The same experiment, as well as some of Priestley's, on the action of hydrogen on the blood, is also referred to by Seguin, in order to prove the absorption of hydrogen as stated above; Ann. Chim. t. v. p. 266, 7. See also Crawford, p. 147.

immediately previous to the passage of the blood through the lungs. The properties and uses of the chyle will be fully considered hereafter; but I may remark in this place, that there can be no doubt that it is the substance destined for the support of the system, by which the waste of the body is repaired, and materials are furnished for its growth and increase. Hence we are led to the conclusion, that arterial blood becomes venalized, not in consequence of any thing which it receives while it is passing into the veins, but from what it loses in forming the various secretions, or in contributing to the growth and nutrition of the body.

Mr. Ellis's hypothesis, respecting the origin of the carbon which is employed in respiration, differs essentially from the opinion that is generally adopted on this subject, in supposing that the carbon does not proceed immediately from the blood, but that it is an excretion, produced by the action of the exhalent vessels of the lungs. These vessels, he conceives, possess the power of discharging both carbon and water; the former, as it may be presumed, in a gaseous, although uncombined state, and that it then meets with the oxygen of the air in the vesicles, and forms carbonic acid. The principal arguments which he adduces in favour of his hypothesis are, that a gas cannot penetrate the cellular or vascular structure which separates the blood from the air in the cells of the lungs; that we have no proof of the existence of any gas existing in the blood, while he insists much upon the analogy between the functions of the

skin and lungs, and considers it as proved, that although the skin converts oxygen into carbonic acid, no gas is either absorbed or discharged from it.⁷

I think it may be stated, in opposition to Mr. Ellis's doctrine, that we possess direct experiments in proof of the action of oxygen on the blood out of the body, where no vital operation can take place, and that this action is not prevented by the operation of a membrane much thicker than that which constitutes the vesicles of the lungs; that it is not oxygen or carbonic acid in their gaseous, but in their solid form, that are supposed to exist in the blood, while the action of the skin upon the air is itself too little understood, and the results of the experiments too uncertain, to warrant us in drawing any conclusion from them.⁸ Mr. Ellis's hypothesis I conceive to be defective, as it does not sufficiently explain the object of the elaborate apparatus, by which the air and the blood are brought into such close and extensive proximity; nor does it show the connexion between the chemical change which the air experiences in the lungs, and the conversion of the blood from the venous to the arterial state. It appears moreover to neglect the analogy which we have between the action of the air on the blood out of the body, and what takes place in the lungs; the change appears to be the same, in each case, both upon the

⁷ Inquiry, p. 195.. 200.

⁸ This remark I conceive to be amply supported by the statements very candidly brought forwards by Mr. Ellis himself, in the second part of his work, p. 353. et seq.

air and the blood, and hence we naturally infer that it is brought about by the same kind of agency.⁹

An objection to the hypothesis which supposes the union of oxygen and carbon to be brought about in the lungs, various facts were adduced to show, that the change from the arterial to the venous state can take place, by the action of the constituents of the blood upon each other, while it remains in the great trunks, in a situation where it is incapable of receiving any addition of extraneous matter. It has been observed in surgical operations, that after a tourniquet has been applied to an arterial trunk, the blood which first flows when we remove the instrument, is perceived to be of the venous colour, and it was remarked by Hunter, that extravasated blood is always purple, even in cases where there is every reason to suppose that it may have proceeded from an artery. That this was actually the case he proved by puncturing the femoral artery of a dog, when upon examining the blood that was effused in the adjoining cellular substance, he found that it was of the purple colour, and as far as could be judged by its external characters, was converted into the venous

⁹ This point is well stated by Crawford, allowance being necessarily made for the discoveries and consequent changes of our hypotheses which have taken place since the date of his publication; On Animal Heat, p. 147, 8. Some late experiments of M. Fodera's on transudation seem very much to favour the idea of the possibility of the air acting upon the blood through the intervention of the vessels. See Magendie's Journ. Physiol. t. iii. p. 35. et seq.

state, although it had been carefully preserved from the contact of any extraneous body. A more direct experiment was then tried; a portion of the carotid of a dog was included between two ligatures, and upon piercing this part of the vessel after some hours, it was found to contain blood which had acquired the complete venous appearance.¹

It was partly from certain facts of this description, and partly from the difficulty which was supposed to exist in accounting for the equable diffusion of heat over the system,² that La Grange formed his hypothesis, which Hassenfratz has illustrated by various arguments and direct experiments.³ According to La Grange, the oxygen is absorbed in the lungs, and enters into a loose combination with the blood, to which it imparts the scarlet colour; during the course of the circulation a more intimate union takes place

¹ On the Blood, p. 65. . 7.

² Hassenfratz, *Ann. Chim. t. ix. p. 265, 6*, observes, that according to Crawford's hypothesis, "les poumons sont le foyer où se degage toute la chaleur que la sang abandonne dans l'economie animale;" and again, "M. de la Grange reflechissant que si toute la chaleur qui se distribue dans l'economie animale se degageoit dans les poumons, &c." It must excite some surprise that these expressions should have been employed on a subject so generally known as Crawford's doctrine of animal heat, which had been many years before the public; and still more so, because Hassenfratz himself, in the beginning of his paper, p. 263, expressly notices the experiments on the different capacities of arterial and venous blood as what were generally recognized. See Mr. Dalton's observations on this point in *Manchester Mem. v. ii. 2d. ser. p. 20*.

³ *Ann. Chim. t. ix. p. 269*.

between the oxygen and the carbon, in consequence of which the blood becomes venalized. The difference, therefore, between arterial and venous blood depends not so much upon the nature and proportion of its constituents, as upon the mode of their combination, and the action which they exercise on each other.⁴ From the time when the blood enters the left auricle of the heart, until it leaves the right ventricle, it undergoes the complete change from the arterial to the venous state, yet it may be presumed that the proportion of oxygen and carbon is not altered, in so far as respects this specific change.⁵

⁴ The experiment of Priestley, in which arterial blood assumed the venous hue by being placed in vacuo, *On Air*, v. iii. p. 364, has been regarded as a proof that the change from the arterial to the venous state must depend upon the action of the constituents of the blood upon each other.

⁵ A modification of La Grange's hypothesis was proposed by Mr. Allen in his lectures on the animal economy, formerly delivered at Edinburgh, according to which a part only of the oxygen, necessary to form the carbonic acid, is united to it in the course of the circulation, so to produce an oxide of carbon; when this arrives at the lungs, it attracts from the air the remaining quantity of oxygen, and is converted into carbonic acid. For an account of Mr. Allen's doctrines on this and some other points connected with it, the valuable thesis of Professor Delarive may be consulted. It is supposed that the serum contains a quantity of pure soda, which is incompatible with the presence of carbonic acid in the blood. The hypothesis of Richerand is very similar to that of Mr. Allen; *Elem. of Phys.* § 76. p. 208. Blumenbach's idea of the nature of the change which the blood experiences is not essentially different from that of La Grange, except that he ascribes the change to carbon only, and not to the compound of carbon and hydrogen; he sup-

In pursuance of this idea Hassenfratz proposed to observe what would be the effect of placing blood in contact with oxygen, or substances supposed to contain it, and also to notice the spontaneous changes which arterial blood undergoes when cut off from all communication with oxygen. The experiments performed with a view to the first of these objects cannot be regarded as entitled to much attention; indeed they principally consist in comparing the effects produced by exposing portions of blood to liquid chlorine, and to muriatic acid. The other set of experiments are, however, more deserving of our

poses that the "oxygenized blood" acquires carbon in the small vessels; *Instit.* § 167. Sir E. Home suggests, as an argument in favour of the opinion that oxygen is actually absorbed by the blood, that if this were not the case, the foetal blood could not be aerated by being brought into proximity with that of the mother; *Phil. Trans.* for 1810, p. 217. The suggestion may be regarded as favourable to the hypothesis, but it might be said that the maternal blood, in this case, merely abstracts carbon from the blood of the foetus. In the same connexion the experiment of Hewson may be mentioned, in which he confined a quantity of blood in the jugular vein between ligatures, and upon admitting air to it, observed that each bubble of air, as it came in contact with the venous blood, converted it to the arterial hue; *Inquiries*, v. i. p. 8, ex. 3. We have an experiment related by Fourcroy, which has been supposed to be favourable to the hypothesis of the absorption of oxygen by the blood; a portion of air was confined in a jar over blood, when the air was found to have its volume diminished and its oxygenous part removed; *Ann. Chim.* t. vii. p. 148, 9; but as this experiment was performed in the infancy of the pneumatic chemistry, we may suspect there is some inaccuracy in the statement.

consideration, both as affording more direct results, and as of so simple a nature as to be little liable to mistake or inaccuracy. Hassenfratz filled a number of tubes with arterial blood, and sealed them hermetically, when he uniformly found that the blood, after some time, lost its scarlet colour, and acquired the complete venous aspect.

Upon the whole the experiments and reasoning of Hassenfratz are not without their value, although few, if any of them, are of that unequivocal nature, as to afford any very direct or decisive proof of the truth of the hypothesis. It must be acknowledged that the mere change of colour which the blood undergoes, when it is extravasated in the cellular texture, enclosed in sealed tubes, and still less the effect of chlorine upon it, can be considered as bearing but an imperfect analogy to what takes place while it is circulating in the vessels. And we may farther remark, that even should we consider the observations and experiments of Hunter and Hassenfratz as proving that the change from the arterial to the venous state may be effected, without any addition *ab extra*, it does not necessarily follow that the reverse operation can take place, nor indeed have we any evidence that it ever has been accomplished, except by the intervention of oxygen. It is more by other considerations, connected with the changes induced upon the air, or with the part which its constituents perform, either separately or conjointly, when placed in contact with the blood, that we must form our opinion upon this controverted subject.

And indeed the merits of the question may be rested almost exclusively upon the single point, whether the oxygen which is consumed be exactly replaced by an equal bulk of carbonic acid, the nitrogen remaining altogether passive, or whether there be a surplus quantity of oxygen absorbed by the blood, as well as a reciprocal absorption and exhalation of nitrogen. After duly balancing the facts and arguments that have been advanced on each side of the question, I have been induced to adopt the latter of these opinions, and as a certain degree of absorption of both oxygen and nitrogen appears to take place in the lungs, there is no difficulty in supposing that this is the case with respect to the whole of what is employed in the system; and we shall probably find it to be more consonant to the other operations of the animal œconomy to conceive of the union between the oxygen and the carbon being brought about during the course of the circulation, than by a momentary contact in the lungs alone.⁶

⁶ We have some observations and experiments of Dr. Davy's, that bear indirectly upon this question, and favour the same opinion. He found that in certain morbid conditions of the chest, the pleuræ appeared to have the power of absorbing, and probably of exhaling air from their surfaces, and this seemed likewise to be the case with air artificially introduced between the pleuræ in their healthy state. Hence he justly infers, that mucous membranes generally possess the property of absorbing and exhaling air, and that these operations are mutually going forward in the natural process of respiration; Phil. Trans. for 1823, p. 496, et seq. An inference of the same kind has been

But this opinion does not rest entirely upon our knowledge of the changes which are induced upon the air by its passage through the lungs; we have some very direct and unexceptionable experiments by Dr. Edwards, which may be regarded as proving both the absorption of oxygen and the exhalation of carbonic acid by the pulmonary vessels. Having shown that the air is diminished by respiration, he proceeds to examine whether the diminution depends upon the absorption of oxygen or of carbonic acid, and he determines in favour of the former, because when a small animal is confined in a large quantity of air, and the process is continued for a sufficient length of time, he found that the rate of absorption was greater at the commencement, than towards the

drawn from the chemical constitution of the air in the swimming bladder of fishes, which must apparently be regarded as the product of the containing membrane, proceeding from exhalation, and probably modified by absorption. It has been found by different experimentalists that the composition of this air differs from that of the atmosphere. It sometimes contains less oxygen, as was found to be the case by Priestley; *On Air*, v. ii. p. 462, 3; but, as it appears, it frequently contains a larger proportion. Biot established this very satisfactorily, by a series of experiments related in *Mem. d'Arcueil*, t. i. p. 252. et seq; from which we learn, that the proportion of oxygen increases with the depth of the water in which the fish usually resides, varying from a very minute quantity to 87 per cent. Biot's experiments have been fully confirmed by Configliachi; *Ann. Phil.* v. v. p. 40. Humboldt and Provençal likewise found that the composition of the air in the swimming bladder of river fish was not uniform in its composition; *Mem. d'Arcueil*, t. ii. p. 400. et seq.

termination of the experiment, while at the former period there must have been an excess of oxygen present, and at the latter an excess of carbonic acid.⁷

Dr. Edwards's experiments in proof of the exhalation of carbonic acid by the lungs are no less ingenious and decisive than those related above. Spallanzani had stated, that when certain animals of the lower orders are confined in gases that contain no oxygen, still the production of carbonic acid is not interrupted; proceeding upon this statement, Dr. Edwards confined frogs in pure hydrogen, in which, by observing the necessary precautions, they are capable of existing for a considerable length of time, while we observe that the action of the lungs is not suspended. The result of this experiment was that carbonic acid was produced, and in such quantity as to show that it could not have been derived from the residual gas in the lungs, being in some cases nearly equal to the bulk of the animal. The same results, although in a less degree, were obtained with fishes, and afterwards with snails, the animals on whom Spallanzani's original observations had been made.⁸

⁷ De l'Influence &c. p. 411, 2.

⁸ Upon these experiments the author remarks, "il est indubitable, qu'elles produisent de l'acide carbonique, en respirant un gaz dépourvu d'oxygène; que cet acide carbonique n'est pas dû à une quantité de ce gaz contenu dans la cavité des organes respiratoires avant l'expérience, ou à l'oxygène qu'ils peuvent renfermer; par conséquent qu'il n'est pas formé de toutes pièces dans l'acte de la respiration, par la combinaison de l'oxygène de l'air inspiré avec le carbone du sang, mais qu'il est le produit de l'exhalation," p. 451.

He also extended his experiments to the mammalia, by taking advantage of a property which he had found to exist in certain species of newly-born animals, of being able to exist, for a short time, without the access of oxygen to their lungs. Kittens of two or three days old were immersed in hydrogen; they remained in this situation for nearly twenty minutes, without being deprived of life, when it was found that they had expired a quantity of carbonic acid greater than could possibly have been contained in their lungs at the commencement of the experiment.⁹

⁹ The conclusion from these experiments is; "que l'acide carbonique expiré est une exhalation qui provient en tout ou en partie de l'acide carbonique contenu dans la masse du sang," p. 465. The experiments related in the text are contained in Dr. Edwards's work, par. 4. c. 16. § 4. p. 437. 465. We have a direct experiment of Legallois' in favour of the absorption of carbonic acid during respiration. He placed an animal in a quantity of air which contained a considerable proportion of carbonic acid, and upon removing the animal, he found an actual diminution in the quantity of carbonic acid; *Ann. de Chim. et Phys.* t. iv. p. 115.

As an indirect argument in favour of the opinion maintained in the text, we may adduce the conclusion which Sir H. Davy formed from his experiments on the respiration of nitrous oxide and hydrogen; "that a certain portion of the carbonic acid produced in respiration is evolved from the blood;" *Researches*, p. 447. We have likewise some farther observations in p. 188.

I must not omit to mention an experiment which has been performed by Magendie and by Orfila, in which when phosphorated oil was injected into the cellular texture or the blood vessels, the phosphorus has been expired in combination with oxygen; See *Mem. by Magendie on Transpiration*, p. 19, 0;

A question still remains to be considered, whether, when the air enters the pulmonary vesicles, it is absorbed in its whole substance, that proportion of each of its constituents which is necessary for the wants of the system being retained, while the excess of each is rejected, or whether the quantity only be absorbed which is afterwards employed, consisting of a large proportion of oxygen and a small proportion of nitrogen. Sir H. Davy thinks that the whole of the air is absorbed, and that the surplus quantity of each of the constituents is afterwards discharged; he remarks that air has the power of acting upon blood through a stratum of serum, and he conceives it probable, that in this case the whole mass must be absorbed before it can arrive at the red particles, upon which its action is specifically exercised.¹ But although this view of the subject appears to be the most probable, yet we must not consider it as resting on any very decisive evidence.²

and Orfila's *Toxicologie*, t. i. p. 531. et seq. In this case it has been supposed by Dr. Prout more probable that the union of the phosphorus and oxygen should take place in the pulmonary vesicles than in the course of the circulation; See *Ann. Phil.* v. xiii. p. 278; but the effect is of so peculiar a nature that it seems scarcely possible to reason from it to what takes place under ordinary circumstances.

¹ *Researches*, p. 447.

² Sir H. Davy's experiments on the respiration of nitrous oxide have been adduced in favour of this opinion, because they have been thought to prove that nitrogen was generated by this process, which it has been supposed could only have taken place by the decomposition of the nitrous oxide after it had been

I have already made some observations on the supposed discharge of hydrogen from the lungs. The experiments and arguments that were employed by Lavoisier, to prove that the water contained in the expired air is generated by the union of oxygen and hydrogen, appear to be totally inadequate to the purpose, and accordingly the hypothesis itself, although at one time so very generally adopted, is at present, I conceive, entirely abandoned.³

As the blood is a very compound fluid, composed of various substances that are loosely combined together, and possess different chemical properties, it has been a subject of inquiry, upon which of its constituents does the air more particularly act. According to the hypothesis which supposes the lungs to be the seat of the operation, the inquiry will be, from what part does the oxygen procure the carbon, and according to the other hypothesis, by what part is

previously absorbed by the blood; *Researches*, p. 412. et seq.; and an argument was drawn from this in favour of the absorption of atmospheric air by the blood. There are, however, several points in these experiments, with respect to the capacity of the lungs in their different states of distention, as well as the relation which they bear to the quantity of air inspired, which require to be re-considered, before we can admit the conclusion that is deduced from them. See note 50 of the *Essay on Respiration*.

³ I may observe, that upon either hypothesis concerning the mode in which the oxygen unites with the carbon, the water was equally supposed to be generated by the union of oxygen and hydrogen, although they differ in the one being a rapid union effected in the lungs, the other a more slow process carried on during the course of the circulation.

the oxygen attracted. Of the two substances into which the blood separates by its spontaneous coagulation, the crassamentum and the serum, the latter appears to be similar, in its chemical relations, to many other parts of the body, and has not been found to possess any specific or peculiar chemical properties,⁴ whereas the former, when employed separately, has the power of acting upon the air in the same manner with the entire mass of blood. Hence therefore we infer that the crassamentum is the great agent in bringing about the change which is effected by respiration. The crassamentum itself is, however, composed of fibrine and red particles, and as the former of these has precisely the same chemical properties with the muscular fibre, which does not appear to possess any relations peculiar to itself, we naturally regard the red globules as that part of the crassamentum on which the air more particularly acts.⁵ Their organization is peculiar to themselves, they are the only part of the blood which is known to possess any specific chemical characters; we have reason to suppose that they are easily decomposed, and are more readily acted upon than either the serum or the

⁴ Berzelius observes, that "serum absorbs very little oxygen;" *Med. Chir. Tr.* v. iii. p. 232.

⁵ *Young's Medical Literature*, p. 503. The curious discovery of Messrs. Dumas and Prevost, that the temperature of an animal is in exact proportion to the quantity of red globules which exist in its blood, may afford an indirect proof of this opinion; *Ann. Chim. et Phys.* t. xxiii. p. 64. et seq. See also Dr. Prout, *Ann. Phil.* v. xiii. p. 270.

fibrine, and it is principally by their change of colour that we are enabled to form our judgment respecting the action of the air upon the blood. The nature of this action is, however, obscure, and we know nothing more than that they appear to have a strong attraction for oxygen, for although it has been shown that they contain a small quantity of iron, there appears no foundation for the opinion, which at one time prevailed, that the iron is the part by which the oxygen is attracted.⁶

Some other circumstances have been pointed out, in which arterial differs from venous blood; it has been stated, for example, that it contains less water and crassamentum. But even, if we admit the facts, which are perhaps not very completely established, this difference might be attributed rather to the effects of secretion and transudation, than to what is to be regarded as the proper action of the lungs. The consideration of these differences between the two states of the blood will therefore be better

⁶ The opinion that the iron in the blood is the constituent on which the air more specifically acts, was generally adopted by the physiologists of the last century, see Haller, *El. Phys.* vi. 3. 18.; and at one time appeared to be proved by the experiments of Fourcroy and Vauquelin, who pointed out the state of combination in which it exists, and the nature of the change which was effected upon it by the air; Fourcroy's *System* by Nicholson, v. ix. p. 207.. 0; but notwithstanding the high authority of these chemists, there appears to have been some inaccuracy in their statement. See the experiments and reasoning of Wells, *Phil. Trans.* for 1797, p. 427. et seq.; also my remarks on the iron in the blood in vol. i. p. 460. et seq.

understood, when we have considered the nature of the secretions, as well as of the substance from which the blood itself is produced, and have compared, as far as is in our power, the chemical relation which these bodies bear to each other. I shall conclude this section by recapitulating the changes which, according to the present state of our knowledge, the blood appears to undergo by respiration, after premising that our information upon this subject is still in a very imperfect state, and that, in most cases, we arrive at our conclusions, rather by indirect inferences, than by any direct experiments that can be made upon the blood itself.

1. The blood, when it leaves the right side of the heart, is of a purple colour; during its passage through the lungs it is converted into a bright scarlet, and again acquires the purple colour when it arrives at the venous part of the circulation. 2. This change from purple to scarlet is effected by the oxygen of the atmospheric air, which is received into the vesicles of the lungs. 3. The same change of colour may be produced upon the crassamentum of the blood out of the vessels by exposing it to atmospheric air, or still more to oxygen, while, on the contrary, scarlet blood is rendered purple by exposure to hydrogen, nitrogen, or carbonic acid. 4. The blood, in passing through the lungs, discharges a quantity of carbon, which is expired in combination with oxygen, under the form of carbonic acid gas. 5. A quantity of aqueous vapour is discharged from the lungs, but this is rather to be considered as the

result of secretion or transudation, than as a proper effect of respiration. 6. The blood, in passing through the lungs, absorbs a portion of oxygen, and this appears to be more than what is necessary for the formation of the carbonic acid which is discharged. 7. It is probable that the blood, as it passes through the lungs, both absorbs and exhales nitrogen, the proportion which these operations bear to each other being very variable, and depending upon certain states of the system, or upon the operation of external agents. 8. It appears, upon the whole, probable, that the atmospheric air is absorbed by the blood in its whole substance, and that certain proportions of each of its ingredients are discharged or retained according to the demands of the system. 9. We have no proof that hydrogen is discharged from the blood.⁷

§ 5. *On the Respiration of the different Gases.*

It may be presumed that no gaseous body, except the compound of oxygen and nitrogen which constitutes the atmosphere, is adapted to the permanent support of life. Of the other gases, there are some which are properly speaking unrespirable, which, on account of the irritation they produce in the upper part of the trachea, it is impossible to take into the lungs; but there are others, which may be received

⁷ I have not entered upon the question respecting the change of its capacity for heat, which the blood has been supposed to experience in its passage through the lungs, because it will fall under our notice, with more propriety, in the next chapter.

into the pulmonary cavities, although their employment is followed sooner or later by some derangement of the system, or even by the extinction of life. The gases upon which experiments of this kind have been made are oxygen, nitrous oxide, nitrogen, hydrogen, and carburetted hydrogen.

The first account which we have of the effect of oxygen, when respired in its unmixed state, is given us by Priestley, who almost immediately upon his discovery of this substance, perceived its remarkable capacity of supporting life, and tried the effect of it upon his own person: he informs us that he felt an agreeable lightness in his chest;⁸ but nothing particular appears to have resulted from the trial, and it may be fairly questioned, whether the sensation which he described is not to be attributed rather to a mental, than to a physical impression. Some individuals who respired oxygen, conceived that it even produced exhilarating effects, while others describe it as giving rise to pain and uneasiness in the thorax. But it can scarcely be doubted, that much of what was described depended upon the imagination, while something was probably owing to the impurity of the gas that was employed, or to the unusual efforts which were made to take it into the lungs. Priestley was also the first who tried the effect of the respiration of oxygen upon animals that were immersed in it; but his experiments went no farther than to prove the superior power which it possesses of sup-

⁸ On Air, v. ii. p. 162.

porting life. He remarks indeed that after an animal had expired in a portion of oxygen, a second animal was able to live for some time in the same air, and it has been inferred from this circumstance, that there must have been something noxious in the gas, which the powers of the constitution were unable to resist for more than a certain length of time. He himself supposed that the death of the mice, the animals which he employed in his experiments, was owing to cold, in consequence of passing through the water with which the gas was confined; he accordingly found that by keeping up the temperature, he was able considerably to prolong the life of the animal, and he remarks, that this experiment completely convinced him that there was nothing in the nature of the gas itself which prevented the mice from living in it.⁹ But another and a more efficacious cause may be assigned for the death of the first animal, that the carbonic acid which was generated, and of which a small portion only would be absorbed by the water, must have acted more powerfully upon the system exhausted by having been for some time exposed to its influence, than upon a fresh and vigorous animal, who would be able for a short period to bear the effect with impunity. Exactly the same occurrence has been observed by Morozzo and others¹ to take place in the respiration of a limited

⁹ On Air, v. ii. p. 165.

¹ Jurine, Enc. Meth. "Medecine," t. i. p. 496; Chaptal's Chem. v. i. p. 131, 2; the experiments of Morozzo on this subject appear to have been made with sufficient accuracy; Journ.

quantity of atmospheric air, and, on the contrary, it has been found by Lavoisier and others, that when the carbonic acid was removed by potash, as fast as it was produced, this effect did not take place.¹

The next account that we have of the respiration of oxygen is by Lavoisier. In his first experiments on this subject he examined the state of the internal organs of an animal, after having been for some time confined in this gas, and he conceived that their appearance indicated that there had been an increased action of the sanguiferous system produced, or something which indicated an approach to the inflammatory state.² We may, however, presume that these appearances must have been the consequence of some accidental cause, for the same philosopher, in his

de Phys. t. xxv. p. 102. et seq.; in one set of experiments a bird lived six hours and a half in a certain quantity of oxygen, and a second lived two hours and five minutes in the same gas; in another set of experiments, the first lived five hours and twenty-three minutes, and others lived in succession in the same gas, as far as the tenth, which lived twenty-one minutes in the same air that had proved fatal to the nine birds that had been previously immersed in it.

¹ We are informed by Dr. Edwards, contrary perhaps to the opinion which is generally adopted, that when warm-blooded animals are confined in a limited quantity of air, they always deoxidate it to the same degree, and that a second animal introduced into the same air expires immediately; *Del'Influence &c.* p. 184. But although the air is ultimately all reduced to the same standard, the effect is produced in very different intervals of time, depending upon various circumstances connected with the constitution of the individuals.

² *Mem. Soc. Roy. Med. pour 1782*, 3, p. 576.

subsequent experiments, which were performed with a more perfect apparatus, and with every appearance of great accuracy, and where the respiration was continued for a much greater length of time, informs us that neither the circulation nor the temperature were affected by it, and in short that no perceptible change was produced by it upon the animal.³ This conclusion is the more worthy of our attention and the more to be confided in, as it not only indicates a change in Lavoisier's opinion, but is unfavourable to the analogy which he wished to establish, between the effects of respiration and combustion.

We have some experiments on the respiration of oxygen by Higgins; he informs us that the pulse was quickened, and that a sensation of warmth was experienced at the chest;⁴ but it may be reasonably inferred that these effects were as much owing to the mechanical method in which the gas was inspired, as to any specific operation produced by the nature of the gas. M. Dumas relates a series of experiments, which he performed on this subject, that were attended with very different effects. He had formed an opinion that the lungs possessed a great degree of irritability, and in order to put it to the test, entered upon the investigation, expecting that the irritability would be rendered more peculiarly obvious by the action of oxygen upon them. A dog was accordingly confined in this gas, and the apparatus was so con-

³ *Mém. Acad. Scienc. pour 1789*, p. 573.

⁴ *Minutes of a Society &c.* p. 144. 6, p. 152.

trived that the air, as it became vitiated, might be withdrawn, and a fresh portion substituted in its place; the process lasted for ten hours, and was resumed after a certain interval, and again resumed for the same length of time for several successive days. The animal now began to be much affected, and various symptoms of disease connected with the chest became manifest, and upon examining the part after death, the lungs were found considerably affected and even ulcerated, so as to exhibit the symptoms of incipient phthisis.⁵

The next account that we have of the effects of the respiration of oxygen is by Beddoes; he performed a series of experiments upon rabbits, in which the attention was particularly directed to the state of the internal organs. He had formed a previous hypothesis, that by the long continued respiration of pure oxygen, a greater quantity of it was absorbed by the blood, than under ordinary circumstances, and that the whole system was, in this way, capable of becoming, as he terms it, oxygenated. The appearances which he found in the animal after death, were accordingly such as seemed very strongly to confirm the hypothesis; the lungs were florid, the

⁵ *Physiol.* t. iii. p. 59. et seq.; the experiments, as the author informs us, were made in the year 1791. Richerand also says, p. 211, that an animal, if long immersed in oxygen, has its circulation quickened, the respiration rendered more frequent, with marks of general excitement. Yet, it is remarkable, he informs us, that notwithstanding these effects, an animal confined in oxygen consumes no more of it than when in atmospheric air; *ibid.*

pleura exhibited marks of inflammatory action, the heart retained its irritability longer than usual; the blood coagulated more rapidly, and indeed the system was so completely saturated with oxygen, that the animals were less easily destroyed by immersion in hydrogen gas or even in water.⁶ These results are in themselves very remarkable, and must appear the more so when contrasted with the opposite statements of Lavoisier;⁷ and it is to be further borne in

⁶ On Factitious Airs, part 1. p. 13. et seq.; p. 38.

⁷ Beddoes quotes the authority of Lavoisier and Priestley in favour of his opinion; on Fact. Airs, part 1. p. 13; and Observations on Calculus, &c. p. 136. 8. We have already seen what is the case with respect to Lavoisier; as to Priestley, the only passage in his works that I conceive can be brought forwards in this connexion, is one in which it is stated, as a mere conjecture, that "as a candle burns out much faster in dephlogisticated than in common air, so we might, as it may be said, live too fast, and the animal powers be too soon exhausted in this pure kind of air." On Air, v. ii. p. 168; but this, although a plausible conjecture at the time when it was formed, can have no weight against the direct experiments that have been since made by Lavoisier and others. The results which he obtained and his opinions upon them were so explicit, that it is surprising Dr. Beddoes should have ventured to adduce them in support of his doctrine of the oxygenation of the blood. I shall quote Lavoisier's remarks at some length. "On sait que la combustion, toutes choses égales d'ailleurs, est d'autant plus rapide, que l'air dans lequel s'opère, est plus pur. Ainsi, par exemple, il se consomme dans un temps donné beaucoup plus de charbon ou de tout autre combustible, dans l'air vital, que dans l'air de l'atmosphère. On avoit toujours pensé, qu'il en étoit de même de la respiration; qu'il devoit s'accélérer dans l'air vital, et qu'alors il devoit se dégager soit dans le poulmon, soit dans le

mind, that the experiments of Beddoes were continued for a considerably less space of time than those of Lavoisier, and likewise that he expressly informs us, that notwithstanding these very singular

écours de la circulation, une plus grande quantité de calorique. Mais l'expérience a détruit toutes ses opinions qui n'étoient fondées que sur l'analogie. Soit que les animaux respirent dans l'air vital pur, soit qu'ils respirent ce même air, mélangé avec une proportion plus ou moins considérable de gaz azote, la quantité d'air vital qu'ils consomment, est toujours la même, à de très-légères différences près. Il nous est arrivé plusieurs fois, de tenir un cochon d'Inde pendant plusieurs jours, soit dans l'air vital pur, soit dans une mélange de quinze parties de gaz azote et d'une d'air vital, en entretenant constamment les mêmes proportions; l'animal dans les deux cas est demeuré dans son état naturel; sa respiration et sa circulation ne paroissent pas sensiblement, ni accélérées, ni retardées; sa chaleur étoit égale, et il avoit seulement, lorsque la proportion de gaz azote devenoit trop forte, un peu plus de disposition à l'assoupissement." *Mem. Acad. Scien. pour 1789, p. 573.* Beddoes's experiments on the respiration of oxygen were performed for the purpose of establishing his opinion, that the morbid state of the lungs in phthisis might be relieved by the respiration of an air less oxygenated than that of the atmosphere. It is remarkable that an opinion of an exactly opposite nature was, about the same time, broached in France, according to which a more oxygenated air was recommended in these complaints, upon the principle, that as the structure of the lungs was injured, they would require an atmosphere from which they might more readily obtain their due proportion of oxygen. The results of the experiments, according to the reports which we have of them, appear to have been equally favourable to each hypothesis; we may fairly conclude that both plans would be equally unavailing. See Fourcroy, *Ann. Chim.* t. iv. p. 83. et seq.; Jurine, *Enc. Méth.* art. "Médecine," t. i. p. 500; Chaptal's *Chém.* v. i. p. 138.

effects which were produced on the system, the oxygen in which the animals had been confined, "seemed to have suffered little diminution either in quantity or quality."⁸ I think that it will not be deemed an uncandid or unfair conclusion to suppose that he was misled by his preconceived hypothesis, and that his zeal in the prosecution of a favourite project, caused him to form an erroneous estimate of the appearances which presented themselves to him, or to overlook some circumstances which would have afforded a more natural explanation of them.

Sir H. Davy has given us the results of some experiments on the respiration of oxygen, and he is induced to infer from them that the long continued employment of this gas would ultimately destroy life; but the grounds on which he formed this opinion are not very explicitly stated, and upon examining the nature of his results, it would appear that the injury done to the system could not arise from the excessive absorption of oxygen, because both when he performed the experiment upon his own person and upon mice, the quantity of oxygen consumed was less than from the use of common air.⁹ Messrs. Allen and Pepys, among their other experiments, tried the effect of the respiration of oxygen; between 3 and 4000 cubic inches of the gas were respired during a period of from 7 to 9 minutes, and the

⁸ On Fact. Airs, part i. p. 13.

⁹ Researches, p. 439. 444. We may presume that this remarkable result was not the effect of inaccuracy or inadvertence, because he expresses his surprise at the circumstance.

result was that a glow of heat was felt over the body, attended by a gentle perspiration. It appeared that during the respiration of oxygen, more carbonic acid was formed than under ordinary circumstances, and also that a portion of oxygen was consumed more than sufficient for the production of the carbonic acid, while it appeared that a corresponding quantity of nitrogen was disengaged from the blood. The proportion of carbonic acid formed during the respiration of oxygen appears to have been 54201·6 cubic inches, while that produced under ordinary circumstances was no more than 39534 cubic inches.¹

I am not aware of any additional information that we have gained on the respiration of oxygen, since the experiments of Messrs. Allen and Pepys. It appears to have been generally taken for granted by physiologists, both in this country and on the continent, that it had a tendency to increase the action of the heart and arteries, and even produce an inflammatory state of the system; but upon reviewing the whole of the evidence, I confess that it does not appear to me sufficient to warrant the conclusion.² With respect to the earlier experiments, it

¹ Phil. Trans. for 1808, p. 265, and p. 277; and for 1809, p. 415. et seq. and p. 427. They found the diminution in the volume of the air to be greater than from the respiration of atmospheric air; in the latter case they assume the average diminution to be about 1-166th, whereas in the respiration of oxygen the diminution in one case was 1-44th. Phil. Trans. for 1808, p. 277.

² M. Magendie, whose opinion may be justly esteemed as a specimen of what is regarded as of the highest authority in France, remarks upon this subject, that pure oxygen is fatal to

may be fairly conjectured that the gas upon which they operated was not pure, from the mode in which it was procured; ⁴ and the experiments of Messrs. Allen and Pepys, which, in this respect are unexceptionable, labour under the disadvantage, which has been already pointed out, that the respiration was not performed in the natural mode, as was the case in those of Lavoisier, where the contrary result was obtained.

Of the gases, which although not capable of supporting life are still respirable, the one which is the least injurious to the system appears to be nitrous oxide. Priestley, who discovered it, supposed it to be highly noxious to animals, ⁵ and the associated Dutch chemists, who afterwards examined its properties, coincided with him in this opinion. ⁶ Sir H. Davy, however, found that this gas may be respired for a short interval without proving fatal, ⁷ and, at the same time, made the curious discovery that the employment of it produces a powerful excitement of the nervous system, in many respects re-

life, and even when mixed in different proportion from that in which it exists in the atmosphere, that it sooner or later causes the death of the animals that are confined in it; *Physiol. t. ii. p. 295.* As he refers to no particular authorities, we may regard this as the current doctrine among his countrymen. Dr. Prout also supports the same doctrine, *Ann. Phil. v. xiii. p. 266*, but without stating the grounds of his opinion.

⁴ We have some useful observations on this point in *Cavallo on Factitious Airs*, c. 9.

⁵ *On Air*, v. ii. p. 55.

⁶ *Journ. Phys. t. xliii. p. 329, 332.*

⁷ *Researches*, p. 333, . 360, p. 425. et seq.

sembling that from alcohol, but differing from it in its not being succeeded by a state of exhaustion.⁸ In addition to his own experience, we have a number of very interesting details of its effects upon other individuals; ⁹ and the experiment has been now so frequently repeated, that although in certain constitutions the peculiar excitement cannot be perceived, and, in some instances, there seems to be even a sedative effect produced,¹ yet no doubt can remain of the general truth of the fact. Sir H. Davy has rendered it probable, that nitrous oxide, when it is taken into the lungs, is absorbed by the blood; he conceives also that it is decomposed by it, an opinion, however, which is scarcely sanctioned by the experiments.

Hydrogen has been frequently respired,² and the

⁸ Researches, p. 456. et seq.

⁹ Ibid. p. 497. et seq.

¹ Thenard, Chem. t. iii. p. 674, 5. gives an account of its effects upon M. Vauquelin. I may add that I experienced the same feelings in my own person; the first inspiration of the oxide produced a sensation like that of fainting, which quickly proceeded to insensibility. Sir H. Davy informs us that the appearances which were exhibited by the lungs of animals that had expired in nitrous oxide were similar to what Beddoes had found in the lungs of animals that had been confined for some time in oxygen, Researches, p. 356, a circumstance which, I conceive, affords a pretty strong presumption that the oxygen employed was not pure; this may perhaps assists us in explaining the extraordinary effects which were supposed to be produced by breathing this gas.

² Scheele, on Air and Fire, p. 160; Fontana, in Phil. Trans. for 1779, p. 337; and Journ. Phys. t. xv. p. 99. et seq.; Pilatre de Rozier, Journ. Phys. t. xxviii. p. 425.

general conclusion which we are led to form is that it possesses no positively noxious effects, and only acts by excluding oxygen, a conclusion which appears necessarily to follow from the experiments of Lavoisier,³ Sir H. Davy,⁴ and Messrs. Allen and Pepys.⁵ A contrary opinion has indeed been maintained by Priestley,⁶ and some other chemists; but it may be presumed that the gases upon which they operated were not sufficiently pure, as their experiments were performed in the earlier periods of the pneumatic chemistry, and we learn from Sir H. Davy,⁷ that a considerable difference of effect is produced according to the method in which the gas is procured.

Pure nitrogen, like hydrogen, appears to act merely by the exclusion of oxygen, an opinion which might naturally be formed respecting a substance that enters so largely into the composition of the atmosphere, for it would seem scarcely possible that any thing which had a positively noxious influence should be at all times received into the lungs in such large quantity. Higgins indeed states that animals die sooner from immersion in nitrogen than from mere interruption to respiration,⁸ but he does not inform us upon what facts this opinion was founded. Sir H. Davy also appears to have experienced a greater sense of

³ Mem. Acad. Scien. pour 1739, p. 574.

⁴ Researches, p. 465, 6.

⁵ Phil. Trans. for 1809, p. 421, 427; see also Beddoes on Fact. Airs, part i. p. 30. et seq. 42.

⁶ On Air, v. i. p. 229.

⁷ Loco citat.

⁸ Minutes of a Society, p. 133.

suffocation from respiring nitrogen than hydrogen, but the gas which he employed contained a portion of carbonic acid,⁹ and he expressly states that immersion in nitrogen or hydrogen proves fatal merely by the exclusion of oxygen, in the same manner with submersion in water.¹ And this view of the subject, if it stood in need of farther support, would appear to be confirmed by the view which we have taken of the action of the air upon the blood, an essential part of which consists in the absorption of nitrogen.

The only remaining gas which is capable of being received into the lungs is carburetted hydrogen. If it be respired in an undiluted state it seems to act as a direct sedative, producing instant death; and if it be employed in small proportion only, diffused through atmospheric air, it induces vertigo, loss of perception, and other symptoms which indicate the extinction of the vital powers.² It acts more rapidly than those gases which merely exclude oxygen, or than the mechanical causes which prevent the admission of this gas into the lungs; and it must therefore be considered as possessing a directly injurious effect upon the animal œconomy. Physiologists are not agreed respecting the mode in which this gas operates; the opinion at one time prevalent, that it acts chemically by abstracting oxygen from the blood, seems to be supported only by a kind of loose analogy, and it is, upon the whole, more probable, that it operates immediately upon the vital powers of the system,

⁹ Researches, p. 466.

¹ Ibid. p. 335.

² Ibid. p. 467. et seq.

destroying either the contractility of the muscles or the sensibility of the nerves, or perhaps both of them, by a direct agency.

All the remaining gases are strictly unrespirable; it is obvious that this must be the case with the irritating acid and alkaline gases, but it may appear remarkable that this should be the case with carbonic acid; although however it must at all times exist in considerable proportion in the air which is contained in the lungs, we learn from the experiments of Pilatre de Rozier³ and Sir H. Davy⁴ that, in an undiluted state, it cannot be taken into the trachea, even by the most powerful voluntary efforts. Sir H. Davy found that air was still unrespirable when it contained three-fifths of its volume of carbonic acid, but that when the proportion was diminished to 3 parts in 10, it might be received into the lungs; the effect which it produced, after being breathed for a minute, was a slight giddiness and a tendency to sleep.⁵

M. Dumas has given us a detail of some experiments which he performed on the respiration of dogs in carbonic acid; his expression would lead us to conclude that he employed pure carbonic acid, but this could not be the case, as the animals lived in the air for a considerable length of time; after death their lungs were found to be in a state which seemed to have been produced by inflammation, but it is very difficult to draw any accurate conclusion from the narrative.⁶

³ Journ. Phys. t. xxviii. p. 422. et seq.

⁴ Researches, p. 472.

⁵ Researches, p. 473.

⁶ Physiol. t. iii. p. 62.

§ 6. *The remote effects of Respiration on the Living System.*

Having now given an account of the direct effects of respiration, I shall next proceed to consider its remote effects upon the living system.

This inquiry must be considered as in fact identical with an investigation into the uses of respiration, for according to the conception which we are led to entertain of the structure and powers of the living body, we conceive that every action which it performs must produce some useful purpose in its œconomy, and be essential to the existence and well-being of the whole. But although no object to which the human mind can be directed is so interesting and delightful as tracing out the final causes of the phenomena which we observe, it has been found by experience that it is not the most appropriate method of arriving at a correct knowledge of the facts themselves. On this account I have thought it more desirable, in a work like the present, to ascertain, in the first instance, as far as the present state of our knowledge will admit of it, the exact nature of the operations of the animal economy, and either altogether to leave the application to be made by the reader, or at least to consider this only as a secondary object of our attention.

The remote effects of respiration will naturally arrange themselves under two heads, those which more immediately affect the vital functions, and those the operation of which is more of a mechanical nature.

Among the remote effects which have been ascribed by the older writers to the function of respiration, there are some, which are so obviously founded upon incorrect principles, that it will not be necessary to enter upon the examination of them, or to do more than merely to allude to them, as forming a part of the history of science. The following, however, are more deserving of our attention, either as being supported by direct experiment, or as having received the sanction of some of the most eminent modern physiologists. The effect of respiration in producing heat, in preserving the contractibility of the muscles, in preventing the decomposition of the body, in promoting the process of sanguification, in the formation of the voice and the various sounds emitted from the larynx, and in the mechanical operations depending upon the motion of the thorax, or upon its connexion with the contiguous viscera.⁷

⁷ Sæmmering enumerates the following uses of respiration; Corp. Hum. Fab. t. vi. § 72. 1. To promote the circulation by a mechanical action, 2. to mix together the components of the blood, 3. to condense the blood by discharging a portion of aqueous vapour, 4. to promote the secretions and excretions by pressing upon the viscera, 5. to assist in chylicification, 6. to enable us to exercise the sense of smell, 7. to enable the infant to suck, 8. to enable the lungs to inhale, 9. it is doubtful whether the body acquires electricity by respiration, 10. by respiration the blood is purified and prevented from putrifying, 11. it maintains the temperature of the body, 12. assists in sanguification, 13. is necessary for the voice and speech. Probably some of the above uses may be thought very problematical; but in addition to them we have the various indirect mechanical purposes which is served by the respiration, which will be enumerated hereafter.

The first of these effects, the production of animal heat, on account of the great extent of the inquiry into which it must necessarily lead us, and still more, from the peculiar, and as it were, specific nature of the operation, I shall consider in the following chapter, as a distinct function. In all warm-blooded animals, where an uninterrupted continuance of the respiration is essential to life, we find that the first effect which ensues from a deficiency of the supply of unrespired air, is the cessation of the contraction of the heart. If we examine the heart when it is in this state, we shall find that its capillary arteries are filled with blood which exhibits the purple venous aspect, and by observing the coincidence between the contractility of the muscular fibres and the nature of the blood which is sent to their minute vessels, we seem to be warranted in the conclusion, that a regular supply of arterialized blood is essential to the support of their contractility, and that the deficiency of this species of blood is the immediate cause of the change which they experience when the respiration is impeded. Goodwyn's observations and experiments were directed to this object, and they fully substantiate his hypothesis, so far as the general question is concerned, respecting the nature of "the connexion of life with respiration."⁸ But the mode of account-

⁸ See the 5th Sect. of his Essay; also Young's Lectures, v. i. p. 739; where the author remarks, that "the muscles are furnished by the blood with a store of that unknown principle, by which they are rendered capable of contracting." Spallanzani goes farther and states that the oxygen which the blood absorbs,

ing for this change will necessarily depend upon the opinion which we adopt respecting the direct effect of respiration upon the blood. If we suppose that this function acts merely in abstracting a portion of carbon from it during its passage through the lungs, we must conclude that the presence of this superabundant carbon in the blood prevents it from preserving the muscles in their contractile state, or if we think it more probable that the oxygen is absorbed in the lungs, we may conceive that the want of contractility is owing to the combined influence of the absence of the ordinary proportion of oxygen and the excess of that of carbon. And whichever supposition we may adopt, it is very possible that some other change may have taken place in the blood, either in its chemical or its mechanical constitution, which may render it no longer fit for the continuance of its appropriate functions, although of the nature of such change we are entirely ignorant. The production of animal heat, should it appear that this is one of the effects of respiration, is essentially connected with the discharge of a portion of carbon, and it will thus follow that these two processes ultimately depend upon the same operation, and that this is to be resolved into a change in the chemical, and possibly also a consequent change in the mechanical nature of the blood.

The share which the nerves have in respiration, or unites with the muscular fibres of the heart and endows it with its contractility; *Memoires*, p. 327; but his hypothesis is deduced from very insufficient premises.

the connexion which subsists between the nervous system and the lungs, has long been a subject of controversy, and has given rise to many elaborate dissertations, as well as to numerous experiments. This question being intimately connected with the effects that result from dividing the par vagum, it may be proper to introduce into this place an account of the facts that have been ascertained, and of the opinions which have been formed respecting this operation.

These nerves,⁹ from the peculiarity in their anatomical relations, have been, at all times, an object of great interest to physiologists. The other cranial nerves are primarily destined to the organs of sense, or to some of their appendages, whereas these nerves are carried to a considerable distance from the head,

⁹ There has been some difference among anatomists in the nomenclature which they have employed with respect to these nerves. The term "8th pair" is considered by many as synonymous with "par vagum;" Boyer, *Anat.* t. iii. p. 350, 1; while others, as it would appear with more accuracy, regard the par vagum, (or as they have been named by the French physiologists, the pneumo-gastric nerves) as only the principal branch of the 8th pair. See the synoptical table in J. Bell's *Anat.* v. iii. p. 113, 4. For a description of the part it may be sufficient to refer to Winslow's *Anat.* Sect. 6. 104. 142 (he terms them *nervi sympathetici medii*); J. Bell's *Anat.* v. iii. p. 153. 9; Sæmmering, *Corp. Hum. fab.* t. iv. § 259; et *De bas. enceph.* in Ludwig, t. ii. § 84. 6; Bichat, *Anat. des.* t. iii. art. 3. § 2. p. 209. 222; and for a delineation of it to Vicq. d'Azyr, pl. 17, 18; to Sæmmering's plate of the base of the brain, for its origin; C. Bell's *Dissect.* pt. 1. pl. 8, and to his engravings of the nerves, pl. 2 and 3.

and are distributed over certain viscera in the thorax and abdomen. This singular destination indicated something peculiar in the functions of the parts to which they are appropriated, while, at the same time, the form and situation of the nerves rendered them particularly favourable for investigating the uses which they serve, as it was easy to deprive the organs of the nervous influence, without the risk of injuring the parts, or affecting them in any other manner. We accordingly find that the division of the par vagum is among the oldest of the physiological experiments that are upon record.

In consequence of the connexion which these nerves have with the recurrents, the effects of their division have been often confounded together, and it was not until comparatively of late years, that we were aware of the difference between their functions, or attempted to separate them from each other. We now know, that by dividing the recurrent nerves, the action of the glottis is deranged, and the voice destroyed or materially impaired, and this was supposed by the ancients to be the chief effect of dividing the par vagum. It was found, however, that other functions were injured, particularly the circulation, the respiration, and the digestion, and that death was, sooner or later, the consequence of the operation.¹

¹ We shall find a very considerable degree of irregularity in the results of the experiments that have been performed on the division of the par vagum. In most cases the death of the animal, or the destruction of some important function, being the evident and direct consequence of the operation, while, in some instances, little more appears to have ensued from it, than

The earlier among the modern physiologists appear, for the most part, to have directed their attention to the effects that were produced upon the heart, and, for a long time, the principal subject of discussion was how far the division of the nerves suspended or destroyed the action of this organ. But although many experiments were performed, and their effects described, the observations were not made with that degree of accuracy, which can enable us to draw any correct deductions from them, nor indeed were the observers themselves aware of the circumstances which it was necessary to attend to, in order to attain a full insight into the subject.²

what might be referred to the pain and irritation produced by the operation. These anomalies are probably, in a great measure, to be referred to the very curious discovery of Dr. Philip, to which I have already alluded, v. i. p. 253, that when a nerve is divided, and the ends remain in opposition, or even when they are separated from each other by a small interval only, the nervous influence continues to be transmitted along it with little or no interruption.

² We have a very interesting historical detail given us by Legallois of all the experiments which have been performed on the par vagum, from the time of Rufus (who appears to have been the first anatomist, who tried the effect of dividing or compressing these nerves) to the date of his own publication in 1812. I have thought it sufficient to notice those only which were made for the purpose of establishing some new principle, or which lead to some important conclusion. See his work "*Sur le Principe de la Vie*," p. 164 et seq. See also Mr. Broughton's sketch of these experiments, prefixed to his paper in the *Quart. Journ.* v. x. p. 292. We have also a list of the authors, with references, in M. Breschet's paper in "*Archives de Medecine*," Aug. 1823.

One of the earliest among the moderns, whose ideas respecting the nervous system assumed a more matured form, was Willis. He divided the par vagum, in order to obtain a test of the truth of his doctrine, that the involuntary motions of the body proceed more immediately from the cerebellum, and having found, in conformity with his preconceived opinion, that the circulation was considerably affected, he did not particularly attend to the effects of the operation upon the other organs.³

Haller's experiments led him to suppose, that the stomach and the lungs were the organs that were more immediately affected by the division of the par vagum, but he has not explained how the effects were produced, what relation the parts bear to each other, or how they act in causing the death of the

³ Cerebri Anat. cap. 24. p. 127. It may be interesting to recite the names of the physiologists who successively occupied themselves with this inquiry, for the purpose of showing the great interest which was attached to it. The following list is taken from Legallois, p. 170; Chirac, Bohn, Duverney, Vieussens, Schrader, Valsalva, Morgagni, Baglivi, Courten, Berger, Ens, Senac, Heuermann, Haller, Brunn, and Molinelli; it appears that, in these cases, the circulation was the function that was more particularly attended to, and that, in a great measure, with reference to Willis's hypothesis. To these may be added Riolanus, Plempius, Lower, and Boyle, who preceded, or were contemporary with, Willis; Haller refers to his relative Brunn, as having performed many experiments on the par vagum. Haighton's experiments on these nerves will be noticed in a subsequent section.

animal.⁴ With respect to the circulation, it seemed to be the general opinion, that the derangement produced in this function was neither so considerable nor so uniform as it should have been, had the heart been the part primarily affected. And with respect to the stomach, although perhaps no organ seemed to sustain more injury, yet life was destroyed more rapidly than it would have been, had the digestion alone been the function that was suspended.

The lungs were therefore concluded to be the organ, the derangement of which was the immediate cause of death, an opinion that seemed to be confirmed by some experiments of Bichat's,⁵ and now the discussion took place respecting the mode in which the division of the par vagum could act upon the lungs, so as to prevent them from performing their functions. A number of experiments were accordingly made to elucidate this point, which, in consequence of the improved state of physiological science, and of the greater dexterity and precision of the operators, were attended with results, that are much more interesting and satisfactory than those of the older anatomists. We are chiefly indebted to the ingenuity and diligence of the French physiologists for the information which we possess upon this topic,

⁴ Haller's experiments are contained in his work "*Sur la Nature sensible et irritable des parties du Corps animal*;" No. 181, 182, 185, 186, 188. His opinion respecting the effect of the operation is stated in *El. Phys.* iv. 5. 2.

⁵ *Sur la Vie &c.* p. 2. Art. 10. § 1. p. 221., 224.

and more particularly to the investigations of Dupuytren, Dumas, Blainville, Provençal, and Legallois.⁶

The points that were more particularly attended to were to ascertain what is the exact state of the lungs, whether they receive the air as usual into their cavities, and whether the same chemical change was induced upon the air as in ordinary respiration. A preliminary step of the investigation, the importance of which seems to have been first duly appreciated by Legallois, was to distinguish accurately between the effects which ensued from dividing the *par vagum* and the recurrent nerves; ⁷ this he accomplished by opening the trachea below the glottis, so as to ensure a free passage for the air into the bronchia.⁸ The result of the investigation appears to be, that the vessels of the lungs are loaded with blood, and the bronchial cells clogged up with a serous or mucous effusion; that the air is of course only partially

⁶ See Legallois' work, p. 177. et seq. Blainville, although he observed that a certain degree of derangement took place in the lungs, appears to have considered it as not an essential effect of the operation, and ascribed the death of the animal to injury sustained by the functions of the stomach. *Ubi supra*, p. 181. Sæmmering, *Corp. Hum. fab. t. iv. p. 237.* remarks that the compression of these nerves by a ligature produces difficulty of breathing, deafness, vomiting, and that it prevents the food from being digested.

⁷ Or perhaps rather the laryngeal nerves, as we are informed by Dr. Hastings, that, in his experiments, he found little or no dyspnœa by dividing the recurrent nerves, but that it took place immediately upon dividing the laryngeal nerves; Philip's *Inq.* p. 121, note.

⁸ *Sur le Principe de la Vie*, p. 188. et seq.; p. 205. et seq.

admitted into the vesicles, and that it experiences a less degree of change in its chemical composition than under ordinary circumstances. This last point, which bears most directly upon the subject under consideration, seems to be established by the experiments of Provençal,² and in conjunction with the other observations, sanctions the conclusion that is drawn by Legallois. He remarks that the division of the par vagum affects the respiration in three ways, it diminishes the quantity of air admitted by the glottis, it retards the circulation of the blood through the pulmonary vessels, and fills the vesicles with a fluid which prevents the admission of the air into them. Hence we learn, that in these cases, the animal is destroyed by a process which is precisely similar to suffocation or drowning, except that it is less complete, and consequently less rapid in its progress.³

Of the three circumstances mentioned above, we may conclude that the third is the essential cause of the effect which ensues. The first depends merely upon the anatomical relation which subsists between the nerves of the glottis and the main branch of the eighth pair, and may accordingly be obviated, by procuring a free admission for the air into the lungs, while the second is probably to be regarded, rather as an effect, than as a primary cause, arising from the contractility of the heart being partially impaired by

² Ubi supra, p. 182; *Mem. de l'Institut de France, pour 1809, histoire* p. 87. par Cuvier.

³ *Sur le Principe de la Vie*, p. 208. et seq.

the defective action of the air upon the blood. It now therefore remains to inquire, in what way the interruption of the nervous influence can produce the effusion into the vesicles; but upon this point it does not appear that we have any direct facts, which can enable us to form a decisive conclusion; and as it involves the question, respecting the influence which the nervous system has over the function of secretion, I shall reserve the farther consideration of it for the chapter which treats upon that subject. So far, therefore, as the experiments on the division of the par vagum respects the general question of the influence of the nerves over the respiratory organs, it leads us to the conclusion, that this influence is exercised in an indirect way only, depending upon the intervention of certain circumstances, which affect the respiration in consequence of their not allowing the air and the blood to be brought within the sphere of their mutual action.²

² This is nearly the same view of the subject which is taken by Magendie; *Physiol.* t. ii. p. 298. et seq. The numerous experiments that have been lately performed in this country and in France on the division of the par vagum, more immediately refer to the effect of the operation upon the functions of the stomach, and will therefore fall under our consideration in a subsequent chapter; I may, however, remark that they agree with those that have been referred to above respecting the state of the lungs. The experiments of Mr. Brodie afford a good illustration of the point in question, by exhibiting the difference of effect which results from the division of the par vagum, according as it is made in the neck of the animal, or near the termination of the nerves in the stomach; *Phil. Trans.* for 1814. p. 103.. 5.

It still, however, remains for us to examine whether in any part of the process of respiration we can perceive a more direct connexion between the nervous system and the lungs, so to indicate a necessary dependence of the one upon the other, and for this purpose, we must trace out the successive steps of the operation, and inquire into the relation of each of them to the other parts of the system.

The two great actions which constitute the process of respiration are the contraction of the heart, by which the blood is propelled through the pulmonary vessels, and the contraction of the diaphragm, by which the air is admitted into the vesicles. With respect to the first of these, I have already endeavoured to show, that it is produced by the distending force of the blood operating upon a contractile organ, which is entirely involuntary, and, in its ordinary action, is altogether unconnected with the nervous power. The diaphragm, on the contrary, being a voluntary muscle, its contractions proceed upon a different principle. But as the power of volition is conceived to be applied to this part on certain occasions only, where it is necessary to produce some extraordinary effect, it still remains for us to inquire in what way its ordinary contractions are produced; whether by a stimulus acting directly upon a contractile part, as in the case of the heart, or whether the stimulating effect is always transmitted to it through the medium of the nerves.

There are many circumstances, which characterize the contraction of the diaphragm, that, at first view,

might induce us to conclude that it is produced in the first of these ways, and that it ought to be classed among what are termed the vital actions. It commences immediately at birth, and continues ever after to exercise its functions, without interruption, in a constant and regular manner, unlike the generality of the actions depending on the nervous influence, which are excited on certain occasions only, when the specific stimulus is applied. But notwithstanding this analogy, we shall find, upon a more accurate examination of the phenomena, that we cannot refer the contraction of the diaphragm to the first class of functions. In the first place, we do not perceive that there is any stimulus immediately applied to the part, so as to produce the effect. And so far as we can form any judgment of the cause which excites the contraction, it would appear to be one which must act through the nerves, for although we are not very well able to trace the connexion between the cause and effect in this case, yet it would appear that the uneasy sensation which we experience, when the lungs do not receive their due supply of fresh air, is the immediate cause of the contraction of the diaphragm.

This view of the subject is confirmed by referring to the anatomical structure of the parts, and especially to the origin and course of the nerves which are transmitted to the diaphragm. From their situation they are less exposed to injury than many other parts of the nervous system, but it appears, that whenever these nerves are affected by any cause, either morbid

or accidental, the action of the diaphragm is proportionally impaired. We have seen with respect to the heart, that if the structure of the organ itself be sound, and its vessels properly supplied with arterial blood, so as to maintain the contractility of its fibres, its action may be kept up for an indefinite length of time, as long as the blood continues to be poured into its cavities, although its communication with the brain be entirely destroyed. But this is not the case with the diaphragm, for if its nerves be injured or divided, it appears that its contraction is completely destroyed, and its functions suspended, unless it be excited by an artificial stimulus.³ Hence we learn, that we are to consider the muscular power, which is exercised in respiration, as being intermediate in its nature between the action of the muscles which are entirely voluntary, such as those of locomotion, and of those which, like the heart, are completely involuntary. The final conclusion which we must draw from these observations will be, that there is a necessary connexion between the nervous system and the function of respiration, and that, in this respect, it differs from the circulation, which is essentially independent of the nerves, although occasionally subject to their influence.⁴

³ This was remarkably illustrated by the experiments of Legallois, in which upon successively removing the different portions of the brain, he always found the action of the diaphragm to be destroyed, when he arrived at the origin of the nerves which supply this part.

⁴ The observations of Dr. Philip on this point are very judicious, although perhaps there may be some objection to the

The celebrated experiment of Vesalius,⁵ but of which Hooke appears to have been the first to show phraseology which he employs. He clearly points out the cause of the difficulty which Legallois experienced in explaining the phenomena, and reconciling them to his system. According to Dr. Philip's view of the subject the lungs are supplied with perception, or as he terms it, sensorial power, by means of the par vagum, so that it is through their intervention that the uneasy feeling is conveyed to the sensorium, which ensues from the defect of fresh air in the vesicles; Inquiry, p. 207, 268; also Quart. Journ. v. xiv. p. 98. et seq. The anatomical structure of the parts, and the experiments of Legallois, to which he refers, render this opinion probable; yet there is still considerable difficulty in conceiving how the state of the air in the pulmonary cavities can act upon the nerves, and we have yet to inquire into the nature of the connexion between the 8th pair and the phrenic nerves.

The student may peruse with advantage Bichat's remarks; "Sur la Vie &c." p. 2. Art. 10. § 2. p. 228. et seq., the object of which is to show, that it is by an indirect effect that the lungs cease to act in consequence of a cessation of the functions of the brain; also Art. 11. § 2, where he endeavours to prove that the lungs are the intermediate organs, which cause the death of the heart to succeed to that of the brain. With respect to the effect of dividing the par vagum, it is remarkable that he draws a conclusion from his experiments which is directly the reverse of what appears to be the natural deduction from them, ubi supra, p. 224. There is frequently an obscurity in this Author, from his peculiar phraseology, and from his hypothesis of the two lives, but the valuable information which he affords us is well worth the trouble of developing his meaning from the metaphysical language with which it is embarrassed. Mr. Brodie adopts a conclusion essentially the same with the one stated above, that when the function of the brain is destroyed respiration ceases, although the circulation is still maintained; Phil. Trans. for 1811. p. 36.

⁵ See note in p. 55.

the importance, and to deduce from it the just inference, where, by artificially introducing air into the lungs of an animal that had been apparently destroyed by interrupting the respiration, the blood is enabled to undergo its appropriate changes from the venous to the arterial state, while, in proportion as this change is effected, the contractility of the heart is restored, seems to be decisive, in pointing out the connexion between the heart and the lungs, but it does not give us any insight into the nature of the connexion between the lungs and the nervous system. In the case of the heart the connexion is of an indirect kind, and takes place by means of a series of intervening operations, which may be clearly referred to distinct sources, although they become sooner or later necessarily connected together and inseparably united. But when, on the contrary, life is extinguished by a blow on the head, by an injury of the spine, or by any of those causes which do not act upon the blood in the first instance, the immediate effect, as far as the respiration is concerned, appears to be the destruction of the contractile power of the diaphragm, because the nerves which give this organ its power of mechanically contributing to the reception of the air into the lungs, are no longer able to transmit to it their specific influence.

The conclusion, that one of the remote effects of respiration is to produce that change in the blood, which may enable it to preserve the muscles in their contractile state, affords an easy explanation of a circumstance connected with the action of the heart, which has given rise to much discussion; in the

ordinary course of the circulation the right ventricle is always filled with venous blood ; this is then transmitted through the lungs, and is brought to the left ventricle in the arterialized state. As the blood is supposed to be equally the cause of contraction in both the ventricles, it has been asked, how the two sides of the heart can be acted upon in the same manner by blood of such different qualities, or more particularly, how the venous blood can enable the right ventricle to contract, when, in other cases, arterial blood appears to be necessary for this purpose. The speculative physiologists have given us many answers to this inquiry. Some of them have been satisfied with referring it to the action of the vital principle, others have assumed certain specific properties, in the right side of the heart, by which it was enabled to be acted upon by venous blood, an hypothesis which was adopted even by Goodwyn;⁶ others have supposed that there was a kind of sympathy between the ventricles, and others again have imagined there to be something in the mechanical structure of the heart, by which the contraction of one of

⁶ *Connexion of Life &c.* p. 82, 4. This would appear to be the opinion of Scemmering, who says, "Sanguis nempe venosus, aeri non admissus, licet ventriculum cordis pulmonalem stimulare queat, tamen ventriculo cordis aortici incitando impar est." *Corp. Hum. fab.* t. vi. p. 76. and upon the same principle Dr. Philip observes that the two sides of the heart are "fitted to obey different stimuli;" *Phil. Trans.* for 1815, p. 81. See also Nicholls's *Pathology*, p. 71, where the author supposes that the contraction of the ventricle may be affected by the state of the blood which is poured into it.

its parts is necessarily succeeded by that of the remaining part.⁷

To a certain extent the latter opinion must be considered as not without some foundation. Although there is a good deal of intricacy in the mechanism of the structure of the heart, and in the form and disposition of its muscular fibres, yet, so far as the present question is concerned, it may probably be regarded as composing one muscle, and will therefore possess that simultaneous action in its various parts, which belongs to the muscles generally, in consequence of which, when any one part of them is stimulated, the whole is thrown into contraction. But although this principle may operate to a certain extent, it cannot be regarded as the correct or proper solution of the proposed inquiry. The blood which is poured into the ventricles of the heart, and causes their contraction, acts, not by any specific properties which it possesses, but merely by its mechanical bulk, while the blood which imparts contractility to the heart is contained in the coronary arteries, which, like the other muscular arteries, proceed from the great trunks in their vicinity, and are distributed through the whole substance of the muscles. Provided the blood in these vessels be properly arterialized, it

⁷ Haller formed a singular opinion on this subject, that the venous blood was the proper stimulus to the heart. He found that when the blood was made to pass through the right side of the heart, after it had ceased to pass through the left side, the contraction of the right side remained longer than that of the left; *Sur les Part. sens. et irrit. mem. 2. ex. 515. 523. t. i. p. 362. 7.*

is immaterial what kind of blood enters the interior cavities of the heart; we may venture to assert, that if it were possible to carry on the circulation through the other parts of the body, it would be sufficient for the action of the heart, if its ventricles were filled with any fluid of the proper temperature and consistence, and in the requisite proportion.^s

These remarks upon the effect of respiration in promoting the contractility of the muscular fibre, will enable us to understand the immediate cause of death from drowning, suffocation, or any other of those accidents which prove fatal by preventing the access of fresh air to the lungs. When the action of the pulmonary organs was conceived to be altogether of a mechanical nature, the cessation of the motion of

^s This point is well treated by Bichat; *Sur la Vie &c.* Art. 6. § 2; but, I believe, this manner of viewing the subject is not original in him, as has been intimated by some of the French writers; See Cuvier, *Leç. Comp.* t. iv. p. 300. I recollect this doctrine being explicitly taught by Mr. Allen, in his admirable lectures on the animal œconomy, which I had the good fortune to attend in the year 1796 and 1797. Mr. Coleman controverts Goodwyn's opinion respecting the state of the two sides of the heart very satisfactorily; he supports his argument by the peculiarities of the foetal circulation; *Dissert.* p. 40. et seq. This may also be regarded as the legitimate deduction from Mr. Brodie's experiments, in which the action of the heart continued after the destruction of the nervous system, until, in consequence of the respiration being suspended, the circulation also ceased; *Phil. Trans.* for 1811, p. 36. Here the immediate effect depended upon the want of the appropriate change in the blood by the air rendering the muscular fibres of the heart no longer contractile.

the heart in drowning or suffocation was ascribed to a mechanical obstacle impeding the passage of the blood through the lungs, and the heart was supposed to be at rest, because the blood was not duly transmitted to its cavities. We are, however, now assured that no obstacle of the nature formerly contemplated exists; that the first, and indeed the essential effect of submersion is to cut off the supply of oxygen from the blood, so that it can no longer undergo its appropriate change; it is therefore carried into the coronary arteries in the venous state, and hence the heart loses its contractility. The various organs of the body, all of which require for their support a regular supply of arterial blood, are consequently unable to perform their functions, and among others, the brain and nerves lose their sensibility, so that all the powers, both physical and vital, are suspended, and in a short time irrecoverably destroyed. We may consider ourselves as indebted to Goodwyn for the first consistent hypothesis upon this subject; for although his ideas on some of the subordinate points are not altogether correct, he is fundamentally right in his view of the connexion which subsists between life and the function of respiration.⁹ The brain and

⁹ See his Essay, sect. 5. He clearly established the fact, respecting which the opinions of physiologists had been previously much divided, See Morgagni, Epist. No. 19. § 30, that the introduction of water into the cells of the lungs is not a necessary effect of submersion; Essay, p. 10, 14; a fact which is equally important in a theoretical and practical point of view. Goodwyn's experiments upon this point were confirmed by those of Mr. Kite and Mr. Coleman; see Essay on Submersion,

nerves it appears are not directly concerned in the process, and only suffer together with the rest of the system; a consideration which, although it would

p. 3, 4; and on *Suspended Respiration*, p. 82, 3; also *Paris's Med. Jur.* v. ii. p. 35. et seq. On speculating upon the state of the system which is produced by drowning, many physiologists have supposed that death is immediately caused by a derangement of the nervous functions, analogous to, or rather identical with apoplexy. The lungs, it is said, are in a state of forced expiration, and are completely loaded with blood, sent into them by the right side of the heart, which is retained there in consequence of the left ventricle not being able to propel its contents. This mechanical congestion of the blood will be communicated to the whole of the sanguiferous system, but it is supposed that its effects will be more peculiarly experienced in the brain, on account of the nature and situation of the vessels of that part. This doctrine is maintained by Mr. Kite, and although I conceive it to be incorrect, it is stated and enforced with considerable ability; *Essay*, p. 46, 66, 75. Mr. Coleman's observations on the above opinion may be read with advantage, and may be regarded as containing a complete refutation of it; *Essay*, p. 135. . 144; but I may remark, that he appears to lay too much stress upon the supposed collapse of the lungs; p. 148. . 1. We shall find many ingenious observations on the state of the functions, and on their relation to each other, when death is caused by the exclusion of air from the lungs, in Dr. Edwards's chapter on *Asphyxia*, p. 263. et seq. Many of the systematic nosologists of the last century considered the affection produced by submersion as a species of apoplexy; this was the case with Cullen, *Nosol.* t. ii. p. 190.

Since writing the above note, I have met with an account of a series of experiments, performed by Professor Meyer, on the presence of water in the lungs of drowned persons. We are informed that he always found more or less fluid in the lungs after drowning, and that it possessed the colour and other sensible properties of the medium in which the animal had been

seem to be a very obvious consequence of the admitted facts, has not been always borne in mind, and has tended to throw an air of mystery over a subject, which is in itself sufficiently intelligible.

During a certain period of time the powers of life are merely suspended, but the parts not being irrecoverably injured,¹ if the proper means be resorted to,

immersed. He employed water to which the hydrocyanate of potash had been added, and tested the fluid that was found in the lungs with the muriate of iron. It appears that he performed 15 experiments, in which animals were drowned under different circumstances, and always with the same results; *Med. Repos.* v. iii. new ser. p. 436. I may remark that Haller, as the result of his inquiry, thought that a small portion of the fluid in which an animal is drowned, is occasionally found in the lungs, but that fluids do not readily pass into them; *El. Phys.* xviii. 3. 22, 25.

¹ See Hunter, in *Phil. Trans.* for 1776, p. 412. et seq. and the same paper, in an improved form, in his "*Observations on the Animal Economy*," p. 129. 141. The older writers were disposed to admit of a much longer continuance of the state of suspension than the moderns; See Boerhaave, *Prælect.* § 203; *Parr's Dict. Art.* "Submersion;" also the long and desultory article "Noyés," by Vaidy, in *Dict. Scien. Med.* t. xxxvi. p. 393. et seq. There is likewise considerable difference of opinion respecting the period during which the respiration can be suspended, without the interruption of any of the functions. Here, as in the former case, we have every reason to suppose, that the narratives which we have of the length of time that divers can remain under water, are much exaggerated. Boyle, referring to the stories of divers remaining under water for three or four hours, informs us, that a person, who was in the habit of diving, for the purpose of recovering goods from sunk vessels, confessed that he could not remain longer than two minutes under water; *Works*, v. i. p. 111. Halley observes, that few persons can live longer under water than half a minute, but adds that a pro-

the actions of the system may be restored. These means essentially consist in enabling the blood to undergo its specific change in the lungs, by introducing into them a quantity of air, containing the requisite proportion of oxygen. But the various functions of the body are so connected together, that the suspension of any one of them is necessarily attended with a derangement of the whole machine; and we accordingly find, that after a very short interval, the mere introduction of fresh air into the lungs is not sufficient to restore its action, because the diaphragm, upon the contraction of which this action essentially depends, has now lost its contractility, in consequence of the arteries being filled with venous blood. We have therefore to direct our attention immediately to the state of the circulation, as

fessed diver may, by long practice, acquire the power of remaining near two minutes; *Phil. Trans.* v. xxix. p. 493. Cavallo is disposed to limit the time to a minute and a half; on *Airs*, p. 26, 7. Dr. Edwards, who has particularly directed his attention to the subject, informs us, that some of the best divers in the swimming school at Paris, can remain under water three minutes; *De l'Influence &c.* p. 269. Dr. Paris, *Med. Jur.* v. ii. p. 34, resting his opinion on Mr. Brodie's authority, conceives it very doubtful, if the heart ever continues to pulsate for so long as five minutes after the respiration has ceased; generally the interval is much shorter; in drowning he seems to limit it to a few moments; p. 37. The stories that are related respecting divers, he regards as entirely fabulous; note in p. 34. It is supposed, however, by Roesler, from a series of valuable and elaborate experiments, made expressly to elucidate this point, that the period assigned by Mr. Brodie is too limited; See *Ed. Med. Jour.* v. xxiii. p. 207, et seq.

well as of the respiration, and we attempt to induce the contraction of the heart by stimulants applied to it, or to any other part of the system, which may indirectly affect the action either of the muscles or of the nerves.² Of these stimulants the most effectual

² The remarks and directions of Hunter, in the papers referred to above, are for the most part very correct and judicious; but he has fallen into one error of importance, in recommending the application of stimulating vapours to the interior of the lungs; *Observ. on Anim. Econ.* p. 136. Fortunately for the patient, the natural actions of the organs are commonly sufficient to exclude the vapours, for if they were admitted, in any considerable quantity, suffocation would be the consequence. The only use of stimulating vapours, is to excite the nerves of the nose, which, by their connexion with the respiratory nerves generally, may eventually stimulate the diaphragm to contraction. The directions published by the Humane Society, in their latest advertisements, appear generally judicious; some of the means formerly employed were at least of very dubious utility. See a good abstract of the same in Rees's *Cyclopædia*, Art. "Drowning." Dr. Gibney's proposal to employ steam as a speedy and effectual method of restoring the heat of the body, may, I conceive, be frequently employed with advantage; On *Vapour Baths*, p. 135, 6. It has been justly remarked, that there are certain cases of suspended animation, as in suffocation from carbonic acid, and in fainting, where the contraction of the heart and diaphragm are restored by applying cold to the surface of the body, and when warmth would be unfavourable, thus producing an exception to the general rule of treatment. But the cases when cold is to be applied are those in which the heat of the body has not been abstracted by a cold medium; if the temperature be much reduced, it will be necessary to proceed in the usual manner, with the application of warmth. I may remark, that in the recovery from apparent drowning, the contractility of the muscles, as exhibited by the action of the heart and diaphragm, is re-established for a considerable time

is caloric, either as applied to the surface of the body generally, by placing it in a warm medium, or by a topical application of it to the region of the stomach, or to any other part more particularly sensible to its influence. With the same intention we apply frictions, and occasionally more powerful stimulants, such as the electric or galvanic shock, transmitted through the heart or the diaphragm, which, if judiciously used, would seem to be indicated, by the power which they are known to possess of exciting the contraction of a muscle, when it can no longer be acted upon by any other means. It is almost unnecessary to add that in the above remarks, I have proposed only to state the general principles upon which we ought to proceed in attempting to restore the powers of life, when they have been suspended in consequence of the want of the due supply of oxygen to the lungs; a variety of minute, although very important considerations, will naturally require our attention, depending upon the peculiar circumstances of each individual case, which must be left to the judgment of the practitioner.³

before the restoration of the sensibility, thus illustrating the general position of the independence of the former of these powers upon the latter. An observation, the converse of the above, tends to illustrate the same position, that when suffocation takes place, from any cause, volition ceases for a considerable time before life is irrecoverably extinguished. See Edwards de l'Influence &c. p. 269.

³ So little conception had Boerhaave of the real objects of respiration, and consequently of the cause of death by submersion, that he conceived an animal might be rendered amphibious, by frequently plunging it while young into water, and

The opinion that one of the remote effects of respiration is to prevent the decomposition of the blood, and eventually that of the body at large, may thus prevent the closing of the foramen ovale and the ductus arteriosus. Why we cannot live without respiration he says, "nobis κρυπτον est, quod forsitan nobis, vestroque ævo, aliquid pateſcet." Prælect. not. ad. § 691. t. v. par. 2. p. 186. Boerhaave's proposal might be pardoned at the period when it was advanced, but we can scarcely extend our apology to Beddoes, who, notwithstanding all the discoveries that had been made since the time of Boerhaave, seriously advances the opinion, that "by frequent immersion in water, the association between the movements of the heart and lungs might perhaps be dissolved; and an animal be inured to live commodiously, for any time, under water." On Fact. Airs, part. 1. p. 41. This opinion had been previously maintained by Buffon; and although it is altogether without foundation, yet in endeavouring to prove it by experiment, he discovered a curious fact, which has been since confirmed by Legallois, and Dr. Edwards, that a newly-born animal can live without air, for a much longer space of time, than an adult of the same species. See Edwards de l'Influence &c. part. 3. chap. 4. p. 165. 174. Buffon made his experiments upon dogs. Legallois and Edwards used rabbits, cats, and other animals of various kinds, and found that they could bear submersion in water for nearly half an hour without injury; it was perceived, however, that this faculty was soon diminished, and that after some days, it was entirely lost. Dr. Edwards also found that it was only certain species of animals which possessed it, and he made the very important observation, that it belongs to those only which have but little power of generating heat when newly-born, a circumstance which seemed exactly to correspond with this capacity of remaining for a certain space of time without air. Sir A. Carlisle observes that animals which hybernate are less easily drowned than others, and gives us the results of some experiments on hedge-hogs in proof of his position; Phil. Trans. for 1805. p. 19.

be considered as having originated from the hypothesis of Crawford,⁴ respecting the source of the inflammable matter employed in the production of animal heat. It is generally admitted, that all the matter which enters into the composition of a living organized body, is subject to perpetual change, that after having performed its appropriate functions, it becomes, in some way or other, altered in its nature, so as to be no longer suitable for the purpose, and that it is then discharged from the system. Now it has been supposed that it is by the exchange of old for new particles that the body is preserved from decomposition, and that when this process is suspended by death, so that the effete matter can no longer be carried off, a complete decomposition ensues. The blood is the medium by which this mutual interchange is effected, the veins are the channels by which the matter is carried off, and the lungs are the organ by which it is finally discharged. Of all the constituents of the body, the blood, and more especially its red globules, appears to be the part which is the most subject to decomposition, and it is accordingly on this, that the air is conceived more immediately to act in the process of respiration: it may be farther observed that the first

⁴ The same opinion had indeed been previously suggested by Priestley, see note 8, p. 69; and even the older physiologists, as may be found in the references to Haller, *El. Phys.* viii. 5. 20, entertained some imperfect ideas of the same nature, but it was not brought into a consistent form until the publication of Crawford's work.

step in the spontaneous decomposition of animal matter consists in the loss of a portion of its carbon, which unites with the oxygen of the atmosphere, and forms carbonic acid, as is the case with the air in the lungs.⁵ Hence it would appear to be not an unfair conclusion, that the cause which more immediately operates in preventing the decomposition of the body, so far at least as the chemical nature of the substances is concerned, consists in the abstraction of a part of the carbon of the blood, and that if these particles were not removed from it in proportion as they are deposited, they would produce a tendency to decomposition, which would terminate in complete disorganization.

These remarks may assist us in forming some judgment respecting the value of an hypothesis which has been very generally adopted, that the power which the living body possesses of resisting the tendency to decomposition, is to be ascribed to the operation of what has been termed the vital principle. If we examine the body immediately

⁵ Spallanzani found that the bodies of the different classes of animals, worms, insects, fishes, oviparous quadrupeds, birds, and the mammalia, all deoxidate the air after death, some of them as much as during life; and this appears to have been the case before any visible marks of decomposition could be observed; *Mem. sur la Respiration*, p. 63, 70, 74, 302, 316. He found the same change to be produced in the air by torpid animals, although the respiration appeared to be entirely suspended; p. 77, 108, 185. The observation, that the decomposition of the animal matter produces carbonic acid, appears to have been first made by Priestley; *On Air*, v. iii. (1st ser.) p. 340, 1.

after death, its structure and composition, so far as we can perceive, is precisely similar to what they were previous to dissolution, yet it soon begins to exhibit a series of chemical changes, which will eventually proceed to its complete destruction, while, if life had continued, it would have retained its form and composition for an indefinite length of time. This difference has been said to be owing to the presence or absence of the vital principle, an agent which is supposed to keep every part of the system in its perfect state, and to regulate all its functions, while conversely, the continuance of this perfection and regularity has been assumed as an evidence of the existence of this principle.

There are two senses in which the term principle has been correctly applied in natural philosophy; first, when we wish to designate a material agent, which produces some specific effect, as, according to the doctrine of Lavoisier, oxygen is said to be the acidifying principle, and one of the constituents of oak bark is styled the tanning principle; or secondly, we may correctly employ the term principle to signify the cause of a number of phenomena, which essentially resemble each other, and which may be all referred to one or more general laws, as the principle of gravitation on the principle of chemical attraction. We may then inquire how far the term principle can be properly applied to the cause of the phenomena of life.

I feel little hesitation in saying that it cannot be used with propriety in the first sense, to designate

any material agent, notwithstanding the high authority of those physiologists, who maintain the existence of a "*materia vitæ*," and go so far as to describe its visible and tangible properties; or of those, who identify the cause of the characteristic properties of life with electricity or any analogous agent.⁶ Nor shall we find the term principle more appropriate when employed in the second sense, to express the supposed cause of a series of phenomena, which may be all referred to one or more general laws; for according to the explanation which has been given of it, by those who have expressed themselves in the most intelligible manner, the vital principle has been employed to express all those actions, which could not be referred to any other general principle.⁷

⁶ I shall defer the objections which, I think, may be deduced against these, as well as against the other modifications of the material hypothesis, to a subsequent part of my work. At present I shall only remark, that in supporting the doctrine of immaterialism, I disclaim all intention of throwing out any imputation or censure against either the principles or talents of those whom I oppose. Such a proceeding I should regard as highly illiberal, and therefore unworthy of one who professes to feel an interest in the advancement of knowledge.

⁷ In order to show that I have not misrepresented the doctrines that are maintained by many of the modern physiologists, upon the subject of the vital principle, in addition to the works that have been already alluded to, I shall subjoin the following references, with a brief abstract of the opinions of the several authors. Barthez, in the introduction to his "*Nouveaux Elémens*," t. i. p. 15. et note 2, speaks of the vital principle, as what is proved to exist by its effects, but of the nature of which we are ignorant, and adds, that it is to be regarded as like the unknown

Besides the laws of mechanics and of chemistry, we observe in the living body various phenomena which

quantities in algebra. Dumas, *El. Phys.* t. i. p. 61, (1^{re} ed.) in the same manner, likens the vital principle to the letters x y z, as employed in algebra to designate unknown quantities; see also the introduction to his 2d ed., where he farther explains his hypothesis, and vindicates his claims to originality against Barthez. Blumenbach, *Instit. Physiol.* § 30, observes that the vital powers are those which "are not referrible to any qualities merely physical, chemical, or mechanical;" a remark which is strictly correct; but, as we have already had occasion to observe, he speaks of the "vital energy" as an individual agent, and classes together under the title of "*vita propria*," actions which have no bond of union except their being unlike every other. Dr. Park, *Inquiry*, p. 113, says "what the vital principle is I shall not attempt to define; but it certainly does not consist in the functions which depend upon it. It is the cause and not the effect." Plenck, *Hydrologia*, p. 15, does not hesitate to consider the vital principle as one of the elements of which the body is composed. Virey, in conformity with the method that has been adverted to, supposes that those animals that have the power of being multiplied by division, or of repairing lost parts, do it by means of "a vital intelligent force," *Histoire des Mœurs, &c.*, t. i. p. 485. The writer of the valuable article "*Anatomy*," in Dr. Brewster's *Encyc.*, v. i. p. 473, 4, employs the vital principle to explain every action that cannot be otherwise accounted for. Dr. Fleming, after enumerating the actions that are peculiar to organized bodies, refers them all to the operation of the vital principle, which he speaks of as an individual agent, yet he designates it only by the negative property of being different from all mechanical or chemical powers, *Phil. of Zoology*, v. i. chap. 2. The term vital principle is frequently employed by Dr. Philip, and although he uses it in a more guarded sense, it is liable to the objections which have been stated above. In his essay in the *Quart. Journ.*, which may be quoted as a matured digest of his physiological doctrines,

essentially differ from these, and which we must therefore ascribe to some other cause; but we find

he says that the vital principle "bestows on bodies certain properties;" v. xiii. p. 97. If by this expression is meant that animate possesses essentially different properties from inanimate matter, no one can deny the position; but if it is intended to convey the idea that the vital principle is something which can be added to or removed from bodies, without affecting their other properties, or to designate a series of phenomena which essentially resemble each other, we are going beyond the limits of correct induction, and are employing a form of speech which has given rise to much misconception and obscurity. Dr. Philip supposes that arterial blood contains the vital principle; *Quart. Journ.* v. xii. p. 20, and v. xiii. p. 112. The only correct meaning of this phrase I apprehend to be, that the blood exhibits those properties which are characteristic of life, viz. that it is contractile and sensitive; for that the blood is connected with the vital actions of the system is a position to which no one can object. M. Dumas, who is a zealous defender of the life of the blood, *El. Phys.* t. i. p. 454. et seq., extends this property to the chyle also; he speaks of it as "*vivant par elle-même*," "*vivant de sa propre vie*;" t. ii. p. 45, 6; and I conceive that we cannot resist the conclusion if we admit the premises. It may be said that the contest merely regards a difference of expression; but in answer to this I reply, that when physiologists state, that certain effects are produced by the vital principle, if the words have any meaning, they must be intended to explain the mode in which the effect is performed, whereas they only tell us that the effect in question is the result of vitality, a proposition of the truth of which no one can doubt, but which affords us no insight into the nature of the operation. Waving, however, any objection that there may be against the term, vital principle, and employing it as synonymous with life or vitality, it will be found that it has been often used in a vague and inappropriate manner. It neither bestows upon a living body its specific properties, nor is it the consequence of these

that these phenomena differ essentially among themselves, so that if we make this want of resemblance the bond of union, we proceed upon the fundamentally erroneous plan of generalizing specific differences, or associating phenomena, not because they resemble each other, but because they cannot be reduced under any other class. We may then conclude, that when it is asserted that the blood resists decomposition in consequence of the operation of the vital principle, if the phrase have any definite meaning, it is saying no more than that the blood is not decomposed because it is contained in the vessels of the living body, an assertion which no one will be disposed to deny, but which unfortunately does not

properties, or the result of organization; but it is by the existence of these properties that life is indicated, and in which life consists. There are certain circumstances in which the living differs from the dead animal; whether these circumstances may be all resolved into the two general principles of contractility and sensibility, is a point for farther inquiry. We have a valuable paper by Ferriar "on the Vital Principle," in which we have an account of the origin of the doctrine and of its successive development; *Manchester Mem.* v. i. p. 216. et seq.; he observes "it is evident that we gain nothing by admitting the supposition, as no distinct account is given of the nature or production of this principle, &c.;" p. 240. Sir Ev. Home makes an observation which cannot be too strongly impressed upon the mind of the physiologist; "it seems," he says, "to be a rule of the animal œconomy that the laws of life should not be employed when the mechanical or chemical laws of matter will answer the purpose;" *Lect. on Comp. Anat.* v. i. p. 477.

throw any light upon the subject of our investigation.

I conceive that the present state of our knowledge does not admit of our giving a satisfactory answer to this question, but so far as we are able to understand it, I think it is very evident that it depends upon no single cause or principle, but upon the conjoined operation of many actions, which together constitute life, or by the operation of which the living differs from the dead animal. The regular supply of fresh materials, as furnished by the digestive organs, the removal of various secretions and excretions, and lastly, the abstraction by the lungs of the superfluous carbon and water, effects which depend upon the united action of both chemical, mechanical, and vital actions, are among the various causes which probably all contribute to the ultimate object.⁸

⁸ The doctrines of Hunter on the subject of vitality have had so extensive an influence upon the opinions of the English physiologists, that it becomes a question of no small interest to ascertain them with accuracy. But even the most devoted admirers of Hunter admit, that this eminent physiologist was not fortunate in the explanation of his principles, and that, in justice to his memory, when speaking of his theories, we should not take his literal expressions, but the general scope and tenor of his doctrines. Nor shall we find our difficulties removed by the expositions that have been given of them by his commentators. I cannot but think that Mr. Abernethy has attributed sentiments to Hunter, which are not fairly to be ascribed to him. Identifying, as it appears, his own ideas with those of Hunter, Mr. Abernethy expressly states his opinion that "irritability is the effect of some subtile, mobile, invisible sub-

- The next of the remote effects of respiration which were enumerated above, is the share which it has been supposed to have in completing the process of assimilation. Arterial is said to differ from venous blood in containing a larger proportion of crassamentum, and as we conceive that the crassamentum is immediately produced from the chyle, which enters the vessels just before the blood is exposed to the action of the air, it has been supposed by Cuvier,⁹

stance superadded to the evident structure of muscles, or other form of vegetable and animal matter, as magnetism is to iron, and as electricity is to various substances with which it may be connected." *Inq. into Hunter's Theory*, p. 39. He afterwards develops at some length the reasons which induce him to regard electricity as the immediate cause of the phenomena of life; p. 38..44. Although not exactly a believer in the existence of the animal spirits, he thinks that the nerves contain "a subtle and mobile substance;" p. 69; and he finally concludes that "if the vital principle of Mr. Hunter be not electricity, it is something of a similar nature;" p. 88. Nothing, it will be observed, can be more explicit than Mr. Abernethy's declaration, that life consists in an independent material agent, superadded to the visible corporeal frame, yet I conceive it would be difficult to prove that such was the conviction of Hunter, although he might occasionally indulge in some speculations of this nature. In *Duncan's Med. Com.* v. ii. p. 198..2, we have a brief, but correct summary of Hunter's doctrines, so far as respects the vitality of the blood.

⁹ Cuvier argues, that as respiration separates carbon and hydrogen from the blood, it will leave in it a greater proportion of nitrogen, and as respiration maintains the contractility of the system, it is probable that it does it by leaving a greater proportion of that body in which alone contractility resides; *Leçons*, t. i. p. 91, 2. See also Young's *Lect.* v. i. p. 739;

that the conversion of chyle into fibrine is one important office which is served by the lungs. Nor is it improbable that there may be a foundation for this opinion, yet it appears, upon the whole, more analogous to the usual operations of the animal œconomy, to ascribe the effect rather to secretion than to respiration; and as to the greater proportion of it in arterial blood, even were the fact completely established, it ought perhaps to be referred more to the abstraction of a portion of the water and serum in the lungs, and to a deposition of crassamentum by the capillary arteries that are distributed over the muscles, than to the production of it in the pulmonary vessels. At the same time we may admit, that the removal of carbon from the blood, during its passage through the lungs, will tend to bring it into that condition which fits it for the purpose of repairing the necessary waste of the body, and maintaining the various functions in their perfect state.

The hypothesis that was adopted by the older physiologists, and which was embraced by Boerhaave,¹ that the blood, in its passage through the lungs, receives its peculiar organization, and especially, that

Thomson's Chem. v. v. p. 629, 0; and Prout, Ann. Phil. v. xiii. p. 278. Van Sweiten, in his commentary to the 97th Aphorism, observes that the air may have some effect in assimilation and sanguification; t. i. p. 136, 7. When the old physiologists speak of a pabulum vitæ existing in the air, they probably attached no very determinate meaning to the expression, but they do not appear to have intended to designate by it a nutritive substance.

¹ Prælect. notæ ad § 200. t. ii. p. 93; § 210. p. 115, 6.

the red globules are generated by the action of the air upon it, while it circulates through the pulmonary vessels, is entirely without proof, and appears to have been formed principally because it was difficult to assign any other effect which the air could produce upon the blood.

There is a singular state of the system, to which certain animals are incident, which is closely connected with the respiration; the apparent suspension of the greatest part of their functions by cold, constituting what has been termed torpidity or hybernation. It appears not to be confined to any peculiar anatomical structure,² nor to any one of the great classes of animals, but seems rather to exist in all cases where the situation or circumstances of the individual render it necessary for them to pass a portion of the year in the torpid state, thus affording us an insight rather into the final, than the efficient cause. It has been generally supposed to bear a close analogy

² Edwards sur l'Influence &c. p. 471, 2; also p. 148, where he gives a list of the animals that become torpid in the climate of France. Sir A. Carlisle has indeed described a peculiar conformation in the great veins of the hybernating mammalia; but this does not appear to be found in the other classes of animals, nor is it very obvious what purpose it serves; Phil. Trans. for 1805. p. 17. See also Fleming's Phil. of Zoology, v. ii. p. 45. et seq. Cuvier informs us that some hybernating animals have certain fatty appendages, connected with the abdominal viscera, which probably serve to retain the heat of the internal parts; but he remarks that this structure is by no means general; Lec. d'Anat. comp. t. iv. p. 91, 2.

to sleep,³ and although I apprehend we shall find that the idea has been carried too far, yet to a certain extent, it appears to exist, and consequently the states of sleep and torpidity may tend mutually to illustrate each other.

Although in the torpid state all the powers and functions are more or less affected, yet it would appear that the respiration is that in which the change is first experienced. In proportion as the animal becomes torpid, the action of the lungs is diminished, until it very nearly, if not altogether ceases,⁴ the circulation, as well as the functions of

³ Elliotson's notes to Blumenbach's *Physiol.* p. 182; Reeve on Torpidity, p. 136.

⁴ Spallanzani, *Mem.* p. 77, 107; Fleming's *Zool.* v. ii. p. 53, 6; Reeve on Torpidity, § 3. p. 21. et seq.; Edwards, de l'*Influence &c.* p. 149. Mr. Ellis has observed this to be the case with snails; the authors who have described the state of torpidity do not quite agree respecting the fact, whether the respiration be entirely suspended when the torpor is complete. On the one hand we might suppose it was the case, as Spallanzani informs us, *Mem.* p. 68, 109, *Rapports*, t. ii. p. 207, that torpid animals are not affected by being immersed in carbonic acid, or other noxious gases. Yet, on the other hand, he says that they slightly deoxidate the air, an effect which he supposes may be produced by the skin; p. 77. I should, however, conceive that the action of the skin could not continue in the living animal, after that of the lungs was suspended. With respect to some of the lower tribes he states, that caterpillars, when perfectly torpid, do not affect the air in any perceptible degree. *Rapports*, t. i. p. 30, 1. Fish, it would appear, do not become perfectly torpid, but, in great degrees of cold, have all their functions weakened, and produce a proportionably less effect

digestion, secretion, and absorption are, in like manner, nearly suspended,⁵ and the temperature is reduced almost to that of the surrounding medium.⁶ Various opinions have been formed respecting the

upon the air; *ibid.* t. i. p. 457. et seq. The accounts that are collected by Dr. Fleming seem to indicate, that in most cases the lungs are not entirely passive, and this may be inferred from the remarks of Reeve. The same was likewise the result of the experiments of Mangili; *Ann. de Mus.* t. ix. p. 106. et seq. But it is not improbable that when the animals are disturbed for the purpose of experiment, a little degree of action may be induced in the pulmonary organs, which did not previously exist there. The results of Spallanzani's experiments on the respiration of various animals of the lower orders, in many of which the effect of temperature was particularly attended to, may be found in the "Rapports," t. i. p. 186, 7; 249, 0; 468. 1.

⁵ Haller, *El. Phys.* xix. 2. 7. We have a curious account by Major General Davies, of the jumping mouse of Canada, during its state of hybernation, which lasts for between seven and eight months, it lies closely rolled up, and completely enveloped in a ball of clay, and in this state lies buried some inches below the surface of the ground, so that during this long period, all means of obtaining nutrition must be effectually precluded: *Lin. Trans.* v. iv. p. 156. et seq. In tab. 8. fig. 6, we have a representation of the perfectly globular form which the animal assumes. There was an opinion current among the Romans, that dormice became fatter during the state of hybernation; see Martial, lib. xiii. ep. 59; but it appears from Barrington, that this is contrary to the fact; we also learn, from the same authority, that the circulation is not entirely suspended during the hybernation of these animals; *Miscellanies*, p. 167, 8.

⁶ Hunter on the Anim. Econ. p. 111. et seq.; Carlisle, *Phil. Trans.* for 1805, p. 17; Fleming's *Zool.* vol. ii. p. 50. et seq.; Reeve on Torpid. p. 12.

cause of hybernation, for although there seemed to be a necessary connexion between the application of a diminished temperature and the torpidity of the functions, yet we were not able to explain why certain animals only experienced this effect from cold, or how they were able to bear this suspension of all their functions, without the body being decomposed, or its powers being irrecoverably destroyed.

An explanation of the first part of the difficulty appears to be afforded us by the experiments of M. De Saissy, who found that hybernating animals possess the power of producing heat in a less degree than other animals with warm blood,⁷ so that when the atmospheric temperature falls below a certain standard, which is uniform for each species, their animal heat declines to a degree which is unable to support their contractility, and of course all the functions that depend upon it. What peculiarity it is in these animals which enables them to maintain their vital powers in the dormant state, seems still very difficult to explain; but this subject will be considered with more advantage when we have proceeded farther in our examination of the various functions.

There is another curious inquiry connected with the function of respiration, how is the change in the blood effected in the fœtus? The fœtal heart is supplied with blood, which exhibits the arterial pr

⁷ Edwards, d'Influence &c. c. iii.

perties,⁸ and gives the muscular fibre sufficient contractility to maintain the circulation, before the air can have access to the lungs; whence then does the blood, in this case, acquire its oxygen, or by what means does it discharge its superfluous quantity of carbon? This point has been long the subject of discussion among physiologists, and notwithstanding some important observations that have been made respecting it by the moderns, we shall find that it still requires farther investigation. The lungs of the fœtus are in an imperfect, or rather in a partially developed state, and must be regarded as one of those organs, the object of which is prospective. I have already had occasion to describe the peculiarity of the fœtal circulation, which essentially consists in a small portion only of the blood being transmitted through them;⁹ we shall also find that the blood does not experience that change in them which is effected after birth, while, at the same time, we

⁸ Although this is the opinion which is, I believe, generally entertained, yet it is necessary to observe that there are some physiologists of great eminence, who do not admit that any difference can be observed in the different parts of the fœtal blood, corresponding to the arterial and venous states. See Bichat, *Sur la Vie &c.* p. 190, 1; and *Anat. gen.* t. ii. p. 344; Cuvier, *Leçons*, t. iv. p. 298; Berzelius, *Anim. Chem.* p. 41; Young's *Med. Lit.* p. 505; and Magendie, *Physiologie*, t. ii. p. 438. I cannot but feel surprise at such an opinion, as in some cases where I have had an opportunity of examining the fœtus immediately after its extraction from the uterus, the different colours of the blood in the funis appeared quite obvious, thus agreeing with the observations of Dr. Jeffray; *De Placenta*, p. 41.

⁹ Vol. i. p. 366. et seq.

observe this same change to take place in a different organ. The fœtus is connected with the mother by a large cellular mass, spread over the internal surface of the uterus, and which consists essentially of two parts, termed from their connexions, the fœtal and the maternal placenta, each of them being attached to the circulation of the fœtus and the mother respectively, but which, so far as we are entitled to judge from various experiments that have been performed expressly to decide the question, have no direct vascular connexion with each other.¹ We find,

¹ Haller, *El. Phys.* xxix. 3. 28; Blumenbach's *Inst. Physiol.* § 575, p. 320; Magendie, *Physiol.* t. ii. p. 443; *Monro's (tertius) Elem.* v. ii. p. 608; See also Darwin's *Zoonomia*, v. i. sect. 38; Jeffray, *de Placenta*, p. 32; Murat, *Art. "Fœtus,"* in *Dict. Scien. Med.*, where the reader may find a more copious than select list of references. MM. Prevost and Dumas announce the curious discovery, that the red globules in the blood of the fœtus differ in their form and volume, from those of the adult, the former being double the size of the latter; this fact, if it be confirmed, must decide the question respecting the vascular communication in the negative; *Ann. Chim. et Phys.* t. xxix. p. 108. The instances, which are not frequently met with, of extra-uterine fœtuses, appear to furnish a strong argument against the existence of a direct vascular communication. The fœtal placenta, in these cases, attaches to any part of the abdominal viscera to which it is contiguous, and in this state the fœtus grows and is nourished, although it is obvious that it can have no vascular connexion with the organs to which it is attached. An example of this kind is detailed by Dr. Baillie, with that perspicuity and correctness which are so characteristic of the author; *Works*, by Wardrop, v. i. p. 226. et seq. Such cases afford us very interesting examples of the powers of the system to adapt themselves to extraordinary circumstances, in consequence of their assuming vicarious functions.

however, that the blood which is sent from the fœtus along the umbilical cord to the placenta in the venous state is returned in the arterial state, so that we are justified in concluding that this organ supplies the place of the lungs, or produces the chemical change in the blood which fits it for the support of life.

It is no doubt difficult to conceive how this can be accomplished without a direct vascular communication, and we can only account for it by supposing that the minute vessels of the placenta, like those of the lungs, are capable of absorbing and exhaling through their coats, the substances necessary for effecting the change, the arteries of the maternal placenta exhaling oxygen and absorbing carbon, while those of the fœtal placenta perform the reverse operation.² It must farther be remarked, that a small

² Mayow appears to have been the first who entertained a correct opinion respecting the use of the placenta, as an organ supplementary to the lungs; he also extended his views to the chick in ovo; Tract. p. 131, et seq. He had not, however, a very clear conception of the manner in which the nitro-aerial particles were obtained by the blood of the fœtus, or by the fluids of the egg, p. 313, 318, 322. Ray states his opinion on this point very clearly; he says that the fœtus "receives air . . . from the maternal blood by the placenta uterina . . ." an opinion which he informs us he obtained from Dr. Ed. Hulse; Wisdom of God &c. p. 73. This doctrine, after that period, seems to have been almost forgotten or neglected, until near the end of the last century. Before that time the placenta was regarded as an organ of nutrition, and the great subject of controversy was, whether the fœtus was nourished entirely by this organ, or

quantity of effect is all that we require in the case of the foetus, some of its functions being not yet called into action, and the remainder acting only in a very limited degree, the mother still supplying the wants of the foetus and superseding many of those

by the placenta in conjunction with the mouth. A good view of the state of opinions in the earlier part of the last century may be found in two papers in the 1st and 2d vol. of the Edinburgh Medical Essays, by Gibson, vol. i. p. 171. et seq., who maintained that the foetus is nourished, partly by the placenta and partly by the liquor amnii, and by Monro, Primus, vol. ii. p. 121. et seq., who argued that the placenta is the only organ concerned. Neither of these writers had any idea of the placenta being an organ supplementary to the lungs, nor do their contemporaries in general seem to have been aware of the necessity of any such organ. I may remark that Mayow supposed the placenta to perform the office of nutrition, as well as to produce the appropriate change in the blood; p. 319, 322. Some physiologists who conceived it necessary that the blood should be purged or purified, as they expressed it, during the course of the circulation, supposed, from the great size of the liver, that it performed this office. Haller discusses the question concerning the share which the placenta has in the nutrition of the foetus, *El. Phys.* xxix. 3. 10, 1; he dismisses the inquiry respecting any farther use which the placenta may serve in a very few words, and although he refers to Mayow, he does not attach any importance to his doctrine, § 37. Sir E. Home gives us an interesting account of the mode in which the blood of the foetus, in the various classes of animals, is enabled to undergo its appropriate change, either by being exposed to the action of the atmosphere, as in the case of the eggs of birds, of water containing a portion of air dissolved in it, as in various species of aquatic animals, or of the arterial blood of the mother, as in the mammalia; *Phil. Trans.* for 1810, p. 213. 7.

causes of expenditure which exist in the animal after birth.³

³ A very clear description of the modern doctrine on this subject is given by Dr. Jeffray, in his *Thesis de Placenta*; a work which contains a judicious summary of the opinions that had been previously entertained upon the subject, together with many important original observations. We have some valuable remarks in Bichat; *Anat. gen. t. i. p. 348*; and *Sur la Vie &c.*, p. 82. et seq.; mixed up, however, with much incorrect hypothesis, depending upon the metaphysical ideas which he entertained respecting the relation between the vital functions. Mayow was quite aware of the degree in which the functions of the mother superseded those of the fœtus, p. 322; he limits the actions of the fœtus almost exclusively to the power of supporting its muscular contractility, and I may remark, that if an arterialized state of the blood be necessary for muscular contraction, it is essential that the fœtal blood should experience an equivalent change, to maintain the action of its heart. We have some judicious observations by Mr. Coleman on the state of the fœtal circulation, as connected with its other functions, *Dissert. p. 46. et seq.*; See also Legallois, *Sur la Vie*, p. 248, 9, on the fœtal functions. Dr. Edwards found the temperature of a seven months' child to be only $89\frac{1}{2}^{\circ}$; *De l'Influence &c.*, p. 236.

Since writing the above remarks, I have perused, in the last number of the *Edinburgh Medical Journal*, an account of the experiments of Dr. Williams, of Liverpool, the object of which was to prove the existence of a direct communication between the sanguiferous vessels of the mother and the fœtus. He operated upon dogs, and the plan which he pursued was to open a pregnant animal, immediately after it had been deprived of life, while the capillaries might still be supposed to retain their contractility, and to inject oil of turpentine coloured with alkannet root into the descending aorta. When the blood of the mother was supposed to be sufficiently impregnated with the oil, one of the pups was removed from the uterus, and its vessels

The state of the chick during incubation, although differing so considerably in its anatomical structure

being opened, a portion of the oil was found to have entered into them. It was detected either by suffering the blood of the *foetus* to drop upon paper, to which it imparted a greasy stain, or the vessel was opened under water, in which case small globules of oil were observed floating upon the surface. Being in Liverpool in the autumn of 1824, in company with Dr. Roget, we were present at one of these experiments. This, however, as Dr. Williams candidly admits, was not successful, owing, in a great measure, as he supposed, to the size of the animal upon which he operated, being too large in proportion to the syringe and the quantity of injection which was employed. We also suggested that the result was liable to deception, in consequence of the peculiarly adhesive nature of the oil, which would cause it to adhere to the apparatus or the fingers of the operators, and might thus be accidentally smeared over the surface of the pup, or in some way interfere with the result. Dr. Williams has since endeavoured to obviate this objection by using rape oil, and by afterwards carefully washing the animal in an alkaline solution. An experiment is related, in which these precautions were employed, yet where the oil was still detected in the blood of the *foetus*. The experiment is one which leads to such important conclusions, that I shall offer no apology to the author for making my remarks upon it without reserve. In the first place, the oil does not seem capable of penetrating into the vessels of the *foetus*, unless it be employed in considerable quantity, and injected with considerable force; is there not therefore some reason to suspect that there may have been a rupture of the delicate cellular texture which is supposed to separate the maternal from the *foetal* vessels? 2d. Notwithstanding the care that was taken to wash off the oil, I conceive that it must be very difficult entirely to remove this cause of inaccuracy; it would therefore be desirable to employ some other substance, that is not liable to this objection, which might be dissolved or suspended in the blood of the mother and afterwards detected.

and arrangements of the parts, physiologically considered, bears a close resemblance to the foetus in utero. We have here an organ analogous to the placenta, in the form of a fine net-work of vessels distributed, on the external surface, of the contents of the egg, which receive the blood from the embryo in the venous and return it in the arterialized state, the shell being provided with a number of pores, which permit the air to act upon the blood, and thus enable it to undergo its appropriate change. Hence we find that a free access of air is as necessary to the evolution of the chick as to the existence of an animal with lungs, so that if the egg be completely in that of the foetus, by means of an appropriate chemical reagent. 3d. It seems to be agreed by all anatomists, and is admitted by Dr. Williams himself, that mercury cannot be made to pass from the mother to the foetus, without an obvious extravasation taking place, a circumstance, which is at least a presumption against the existence of any natural passage, through which the oil could pass from one system of vessels to the other. 4th. It is known that when we draw off a large proportion of the blood of the mother, the quantity of blood in the foetus does not appear to be diminished. 5th. The nature of the foetal circulation, both as to the quantity of the blood, the rapidity of its motion, the number of its red particles and other properties are not what would seem to indicate that there is so very minute a channel of communication between the two sets of vessels, if we are to regard them as forming parts of the same circulating system. Should future experiments confirm those of Dr. Williams, the degree of effect would rather indicate some peculiar connexion, essential indeed to the existence of the foetus, but different from the simple circulation of the blood, as it takes place in the other parts of the sanguiferous system.

smear'd over with varnish, the chick is as effectually destroyed, as the animal after birth would be by submersion or suffocation.⁴

There is a subject connected with the effect of respiration on the living system, which must be noticed in this place, both as in itself sufficiently curious to demand our attention, and likewise because it has been supposed to throw light upon the theory of respiration, or upon the mode in which it affects the vital functions. I refer to the peculiar sensations which are experienced at great elevations. These sensations have been supposed to be connected with the action of the lungs, both because a change in the density of the air is the only circumstance to which they can, with any probability, be assigned, and because the respiration appears to be generally affected by any change of this kind to which the lungs are subjected. The effect produced by ascending high mountains was distinctly noticed by Boyle; he ascribes it to the rarefaction of the air, but he

* Blumenbach's Compar. Physiol. by Lawrence, p. 483; Paris on the Physiology of the Egg, Ann. Phil. v. li. N. S. p. 2. et seq. As was remarked above, we are indebted to Mayow for the first clear conception of this subject, although on some minor points his opinion is probably not correct. Sir Ev. Home has given us a series of very interesting engravings, exhibiting the progressive changes which the egg undergoes during incubation; Phil. Trans. for 1822. p. 339. et seq. I must not omit to mention, that in two very elaborate articles in Rees's Cyclopædia, "Egg" and "Incubation," the doctrine maintained in the text, respecting the action of the air upon the blood of the chick in ovo, is controverted in all its parts.

does not satisfactorily explain how rarefied air should produce the feelings which are experienced.⁵ Haller,⁶ with his usual diligence, has collected accounts from various travellers who have ascended high mountains, but the result of their testimony seems to be adverse to the supposition that any peculiar or specific effect is produced in these situations from the state of the air. He also informs us that this was the case with himself, in his expeditions among the Alps; he farther observes, that in many parts of Switzerland, there are individuals permanently residing at very considerable elevations, without experiencing any inconvenience, and it may be added that this still more remarkably takes place in some parts of the E. Indies and S. America.⁷ It is also stated that no effects of an analogous kind have ever been noticed with respect to the different species of animals that are found in these regions. Haller is

⁵ Works, i. 105, 6; iii. 374, 5. The individuals to whom he refers experienced affections of the stomach, as well as of the lungs. It is also noticed by Mead, who ascribes it to extreme rarity of the air, so that enough of it cannot be taken into the lungs to inflate them; Works, v. i. p. 181.

⁶ El. Phys. viii. 3. 7.

⁷ According to Lieut. Gerard, Marang, a large town on the Sutlej, is 8,500 feet above the level of the ocean, and Skipké 9,000 feet, Geol. Trans. v. i. N. S. p. 128, 9; the village of Misang 10,165 feet, Edin. Phil. Journ. v. x. p. 302, and Nako 11,550 feet; he farther states that fields are cultivated at an elevation of 13,000 feet; Brewster's Journ. of Science, v. i. p. 41, et seq. The height of the city of Quito is said to be above 9,000 feet; Jameson's Miner. v. iii. p. 333.

disposed to ascribe the peculiar sensations which have been occasionally felt upon ascending high mountains, rather to excessive fatigue or exhaustion than to any thing specifically depending upon the state of the air, an opinion which had been previously formed by Bouguier, from his experience of what occurred to himself on the Andes.⁸

Saussure, however, who has given us a very minute account of his own sensations on the Alps, has formed a different opinion on this subject, and may at least lead us to doubt the correctness of Haller's conclusion. When he was at the height of above 8000 feet above the level of the ocean, he always expe-

⁸ Saussure, *Voy. dans les Alpes*, t. vii. p. 339; Bouguier, in his abridged narrative of the expedition to Pinchincha, undertaken by Condamine, himself and others, ascribes the effect which some of the party experienced to fatigue rather than to any peculiar state of the respiration; but the symptoms which he relates do not justify this opinion; *Mem. Acad. pour 1744*, p. 261. These travellers spent three weeks on the summit of the mountain, the height of which is above 16,000 feet; it appears that they were less affected after remaining for some time in this highly rarefied atmosphere. In the history prefixed to the volume of the *Mem. Acad. Scien.* for the year 1705, it stated that Cassini and Maraldi experienced no affection of the breathing at an elevation where the atmosphere possessed scarcely more than half of its ordinary weight, p. 15. In the 29th vol. of the *Phil. Trans.* p. 317. et seq. we have an account by Mr. Eden of his ascending the Peak of Teneriffe, an height of above 12,000 feet; the narrative is written in a simple and unaffected style, and it appears from his remarks, that the respiration was not affected; he particularly states that "the report is false about the difficulty of breathing upon the top of this place;" p. 324.

rienced an extreme degree of fatigue and loss of muscular power, and this differed from ordinary fatigue in its coming on more rapidly, being quite irresistible, in being attended with violent palpitations of the heart and beating of the arteries, and it was remarkably distinguished by the very short space of time in which all the unpleasant sensations were removed, so that in two or three minutes he seemed to be perfectly recovered from a state of complete exhaustion, while, almost immediately upon resuming his exertions, the exhaustion, together with the total loss of muscular power, was again experienced. Saussure also mentions, as one of the specific effects of these situations, the great tendency to drowsiness, which is more than proportionate to the previous fatigue, and he remarks that sleep seized them immediately upon their being at rest, notwithstanding all the inconveniences of their situation, and the various circumstances, unfavourable to sleep, with which they were surrounded. He informs us that the affection exists in different degrees in different individuals, and that it is less observable in those who have been long habituated to these situations, although they are not exempt from it.⁹

The accounts that are given us by the late travellers among the Himalaya mountains and the Andes are not very uniform, with respect to the effects of these great elevations upon the respiration or the other

⁹ Voyages dans les Alpes, t. ii. § 559., 561; t. v. § 1280; t. vii. § 2021.

functions of the body. Mr. Moorcroft, in his journey to the lake Manasarovara, unfortunately does not notice the heights of the country through which he passed, but they must necessarily have been very considerable.¹ He informs us that when he was at the village of Niti, his breathing was quickened, and that he was obliged to stop frequently in consequence of the increased action of the heart.² He experienced the same sensations in other places, and it is remarkable that his breathing was often oppressed while he was lying down, and especially, just before falling asleep,³ a circumstance in which it differs materially from what is described by Saussure. The difficulty of breathing was also felt by Capt. Webb, when he visited Niti, and he farther adds that horses are liable to it as well as men.⁴ Lieut. Gerard, who appears to have ascended to greater heights among the Himalayas than any other individual, mentions that he suffered excessive debility and severe head ache, but the respiration does not appear to have been much affected, although he was at elevations of 15, 16, 18, and even of 19,000 feet; indeed in the last case, he expressly states that he attained that great height "without much difficulty."⁵ He, as well as the other travellers among these mountains,

¹ Asiatic Researches, v. xii, p. 375. et seq.

² Ibid. p. 397.

³ Ibid. p. 399, 407, 8, 9, 412, 4.

⁴ Quart. Journ. v. ix. p. 65.

⁵ Geol. Trans. v. i. N. S. p. 124. et seq.; Edin. Phil. Journ. v. x. p. 295. et seq.; Brewster's Journ. of Science, v. i. p. 41. et seq.

informs us that the natives attribute these peculiar effects to the poisonous exhalation or effluvia from certain plants which grow in these regions. Dr. Govan, on the other hand, who crossed the Himalayas at an elevation of considerably more than 15,000 feet, felt nothing peculiar in his respiration or other functions, nor was any one of a train of 40 natives who accompanied him in any way affected; yet he is aware that the sensations are frequently experienced and even at a much less elevation.⁶ Mr. Caldeleugh also, who twice crossed the Andes, at a height of between 12 and 13,000 feet, seems to have experienced nothing more than what might be reasonably ascribed to the fatigue of the journey, nor did any thing particular occur to his guides and attendants.⁷

Upon the whole I think it highly probable that a part of what Saussure experienced depended upon something more than mere fatigue, and that certain specific effects are produced on the system by the rarity of the air. He is himself disposed to account for the phenomena on mechanical principles, depending on the diminished pressure to which the body must be subjected under these circumstances,⁸ while others have thought that the want of a due proportion of oxygen would afford a more easy explanation of them. I think that we are scarcely in possession of

⁶ Brewster's Journ. of Science, v. ii. p. 282.

⁷ Travels in S. America, v. i. p. 309; v. ii. p. 110.

⁸ See also Sauvages, Œuvres Diverses, t. ii. p. 164, referring to the description of Bouguier, who had previously ascribed it to the same cause.

any facts which will enable us to decide upon the merits of the first of these hypotheses; as for the second I conceive that it cannot be maintained, because the effects which have been found to follow from the respiration of air which is deficient in oxygen are different from those described by Saussure.*

We have now considered the various remote effects of respiration, which are more immediately connected with, or dependent upon, a chemical change in the nature of the substances concerned. I must next proceed to give some account of those that are more of a mechanical nature. I have already had occasion to remark upon this subject in the first section of this chapter, when I endeavoured to show that the earlier physiologists, in consequence of their limited knowledge respecting the nature of the air, were disposed to regard the effects of respiration as almost

* Dr. Edwards ascribes part at least of the effect which is produced upon the breathing by great elevations to the increased evaporation which will take place from the skin and lungs; *De l'Influence &c.* p. 493, et seq. The rarity of the atmosphere in these situations, would no doubt tend to promote the evaporation, but, on the other hand, it must be checked by the low temperation, and although, in certain states of the atmosphere, the air of high mountains appears to be peculiarly dry, it is frequently in the contrary state. Dr. Edwards observes that uneasy sensations are occasionally felt when the air of an apartment is rendered dry by the mode in which it is warmed; he is correct in the observation, but it may be remarked, that the feelings which take place in these cases do not seem in any respect to resemble those that are experienced on high mountains; p. 498, 9.

exclusively of a mechanical nature, so that they were generally much exaggerated, while many of them were probably altogether imaginary.

There is, however, one very important function, which is necessarily connected with the lungs, and which may be classed among the remote effects of their action, the formation of the voice.¹ The superior extremity of the trachea is furnished with a number of cartilages of a peculiar form, which constitute the larynx, in the upper part of which is a chink or cleft, called the glottis. The cartilages which compose the larynx are connected to each other by an apparatus of muscles so constructed, that the aperture may have its form and dimensions very considerably varied; and if the air be forcibly propelled from the lungs through the glottis, according to the form and size of the aperture, the different vocal sounds will be produced. The muscles of the glottis are under the control of the will, and we are enabled by our voluntary efforts, operating through the medium of these muscles, to produce all the vocal or musical tones of which the voice is susceptible.²

¹ So little advanced was our knowledge of the nature of respiration, even in the middle of the last century, that Haller supposed the formation of the voice to be one of the principal uses of this function; *El. Phys.* viii. 5. 23.

² An accurate delineation of the muscles of the larynx, and the mode in which they are connected together, may be seen in Albinus's description of the muscles; *Tab.* 11. *fig.* 44. 48; *Tab.* 12. *fig.* 1. 7. A good deal of discussion formerly took place respecting the effect of tying or cutting the nerves that

Speech depends upon another series of actions, connected with the muscles of the tongue and lips, which, although they are distinct from those that

are distributed over these muscles; but it is now generally admitted, that when the nervous communication is entirely intercepted, the voice is destroyed. A part at least of the uncertainty which attached to the subject depends upon the circumstance, that it was thought necessary to operate upon the recurrent nerves alone, whereas these are not the only nerves that supply these parts. Haighton's experiments led him to conclude, "that the recurrent branches of the par vagum supply parts which are essentially necessary to the formation of the voice; while the laryngeal branches of it seem only to affect its modulation or tone." *Mem. Med. Soc.* v. iii. p. 435, 6. *Vid. supra*, p. 163. Probably some of the uncertainty which has prevailed respecting the use of these nerves may be attributed to the curious fact, which was first established by Haighton, that a divided nerve possesses the power of uniting and having its original properties restored; this he proved both with respect to the nerves which are concerned in the voice, *Ibid.* p. 436. .8, and the main trunk of the par vagum; *Phil. Trans.* for 1795, p. 190. et seq. See Blumenbach, *Inst. Physiol.* § 156, p. 89, also note A; Legallois, *Sur la Vie*, p. 187. et seq.; Magendie, *El. Physiol.* t. i. p. 206, also p. 208, et seq. For our first accurate opinions respecting the anatomy and actions of the organs of the voice we appear to be indebted to Fabricius, *De Larynge &c.*, Op. p. 268..317. Martine's paper in *Edin. Med. Essays*, v. ii. p. 114, may be read for the opinions which prevailed on this subject about the beginning of the last century. See Haller, *El. Physiol.* ix. 1. 1..29, for an account of the larynx; also *Monro's (tert.) Outlines*, v. ii. p. 411. et seq., and *Elemen.* v. ii. p. 73. et seq. For the comparative anatomy and physiology of the parts it will be sufficient to refer to Blumenbach, ch. 15. p. 278. et seq.; and to Cuvier, *Lec.* 28. t. iv. p. 445. et seq.

are concerned in the formation of the voice, are, like them, connected with the respiration, as articulate sounds necessarily depend upon the emission of air from the lungs. Besides the cartilages and muscles that compose the larynx, there are several ligaments, which serve to connect the various parts, and which, from their supposed use, have been termed vocal cords. It was a question that has been much agitated, and especially among the French physiologists of the last century,³ whether the musical tones of the

³ The principal writers in this controversy were Dodart and Ferrein; the former endeavoured to prove that the tones of the voice are regulated entirely by the opening of the glottis; *Mem. Acad. pour 1700*, p. 244. et seq. et *pour 1707*, p. 66. et seq.; his first paper contains an account of the opinions of the ancients and the earlier of the moderns. Ferrein, on the contrary, *Mem. Acad. pour 1741*, p. 409. et seq. compares the larynx to a violin, p. 416. or a harpsichord, p. 422. and conceives that the voice is produced by the vibrations of the lips or ligaments of the glottis; he goes so far as to compare the air to the bow acting upon these parts; p. 416. See Haller, *El. Phys.* ix. 3. 1. 17. Blumenbach supposes the larynx to be analogous to the flute, *Inst. Physiol.* by Elliotson, sect. 9. p. 87. et seq. Dr. Young adopts an opinion more like that of Ferrein, that the ligaments of the glottis act like strings; *Lect. v. i.* p. 400, 1.; see also *Phil. Trans.* for 1800, p. 141, 2. Nearly the same doctrine is maintained by Sæmmering, with respect to the vibration of the ligaments, yet he conceives that the larynx resembles a pipe or flute; *Corp. Hum. fab. t. vi.* p. 93. § 94. We have a very elaborate description of the vocal organs and their mode of action, by M. Magendie; *Physiol. t. i.* p. 196, et seq.; both from the form and structure of the parts, and from experiments made upon the dead subject, he is disposed to refer the modifications of the voice principally to the vibrations of the ligaments, and supposes that they are analogous to the reed in the hautbois; p. 207, 8.

voice depend upon the size of the aperture, or upon the degree of tension of these ligaments ; whether the larynx was more analogous to a wind or a stringed instrument. Although it may not be very easy to give a decisive proof of either of these hypotheses, yet if we are to adopt one in exclusion to the other, I conceive that it is more probable that the ligaments serve to regulate the size and form of the aperture, than that they are themselves instruments of sound. A most curious part of the mechanism of the voice consists in the extreme delicacy with which we are able to modify its tones, and the power which we possess of imitating the tones of others. The same observation applies to the organs of speech, but as in this case the parts are exposed to view, we can more

This hypothesis would, however, appear not to be original in M. Magendie, as it is directly controverted by Dodart; *Mem. Acad. pour 1700*, p. 246. We have some judicious remarks on Magendie's hypothesis in the *Ed. Med. Jour.* v. xv. p. 576. See also Good's *Study of Med.* v. i. p. 429. et seq. for the account of the vocal organs and the formation of the voice. I may refer to this author for a concise, but as it appears to me, satisfactory account of the curious art of ventriloquism; p. 435. Some of the older writers had supposed that the individuals who were possessed of this faculty, had an additional organ lower down than ordinary in the trachea, from which the name was taken. Mr. Gough, and others, ascribed it to echoes proceeding from the walls of the apartment, which deceived the audience, and were mistaken by them for the direct impulse of the voice. Dr. Good conceives it to be an imitative act produced by a peculiarly delicate modification of the glottis, while at the same time the sound is generally emitted through the nostrils instead of the mouth

easily conceive how the process of imitation is conducted than when an internal organ is concerned, where the operation is entirely concealed from our sight. But this point will be considered more fully in a subsequent part of the work.

The great diversity of articulate sounds, and the celerity with which we are able to accomplish the necessary muscular contractions, have been frequently commented upon by physiologists, and it may be asserted, that as the gift of speech is one of those powers which eminently distinguishes the human species from all other animals, so there is none in which both the mechanism by which it is produced, and the acquired perceptions with which it is associated, afford a more worthy subject of our admiration.⁴

There are a variety of actions, partaking more or less of a mechanical nature, in which the lungs and chest are essentially concerned, depending upon some variation in their bulk, the extent or velocity of their action, or the manner in which they affect the con-

⁴ In Blumenbach's *Comparative Anatomy*, ch. 15, we shall find the cause why no animal, except man, is capable of uttering articulate sounds; see also Camper's description of the larynx of the orang outang, *Phil. Trans.* for 1779, p. 139. et seq., from which we learn why this animal is in the same predicament. We have a minute account given us by Haller, *El. Phys.* ix. 4. 2. 8. of the different sounds, and an analysis of the particular muscular contractions by which they are each of them produced. Also by Sæmmering, *Corp. Hum. fab.* t. vi. p. 103. § 113. et seq.; by Blumenbach, *Physiol.* sect. 9, § 160. et seq. p. 91. et seq.; and by Dr. Young, *Lect.* v. ii. p. 276. et seq.

tiguous parts. Some of these are, to a certain degree, instinctive, being directly subservient to some useful purpose in the animal œconomy, while they are more or less independent of the will, such as sneezing and coughing. There are others, on the contrary, which are entirely under the control of the will, depending upon the contraction of the diaphragm or the muscles of the chest, which we call into action and regulate at pleasure, like other voluntary actions, such as sucking and straining. Some of these actions may be regarded as modifications of the voice, being characterized by distinctive sounds, essentially connected with their final cause, as laughing and weeping. The mechanical actions connected with respiration, which are enumerated by Haller⁵ and Sœmmering⁶ are the following; sighing, yawning, sucking, panting, straining, coughing, sneezing, laughing, weeping, hiccup, and vomiting.⁷

Sighing consists in a full and protracted inspiration, by which the cavity of the chest is considerably augmented; its final cause appears to be to promote the passage of the blood through the pulmonary vessels and to enable the air to act more fully upon it. Yawning also consists in a full, slow, and long inspiration, but it differs from sighing in being followed by a slow and full expiration; it is also

⁵ *El. Phys.* viii. 4. 30. .40.

⁶ *Corp. Hum. fab.* t. vi. p. 79. § 80. .90.

⁷ Of this list Blumenbach omits sucking, panting, straining, and vomiting. *Physiol. Sect.* 9. § 162. p. 92, 3. He only enumerates those which he regards as modifications of the voice. See also Sprengel, *Instit. Med.* t. i. sect. 4. § 220. .5. p. 490. .7.

attended by an involuntary opening of the jaws, by which the air has a more free admission to all parts of the chest. In sucking we apply the lips closely to the vessel containing the fluid, and by making an inspiration, we encrease the capacity of the chest; the air in the mouth and fauces thus becomes rarefied, and the pressure of the atmosphere causes a portion of the fluid to enter the mouth. Panting consists in a succession of alternate quick and short inspirations and expirations, and thus produces a frequent renewal of the air in the lungs, in cases where the circulation is unusually rapid, or where, from some obstruction in the chest, we require a more than ordinary supply of fresh air. In the act of straining, we commence by a full inspiration, and retain the air in the chest, while, at the same time, we contract the abdominal muscles. By this means we not only compress the viscera, and expel their contents, but the flow of the blood is retarded, and it has a tendency to accumulate in the venous part of the circulation. The act of straining enables us to exercise the greatest degree of muscular power, because the trunk becomes firmly fixed and serves as the point in which the actions of all the muscles are centred. Coughing is produced by a quick and forcible contraction of the diaphragm, by which a large quantity of air is received into the chest; this, by a powerful and rapid contraction of the abdominal muscles, is propelled through the trachea with considerable force, and in this way dislodges mucus, or any other extraneous substance which irritates the part. When the irritation is considerable, it is in-

voluntary, although, in other cases, it is under the control of the will. Sneezing, in many respects, resembles coughing, but it differs from it in being more violent and in being involuntary. The irritation is applied to a more sensible part, the inspiration with which it commences is more deep, and the succeeding expirations are more violent, and are directed through the cavities of the nose. The final cause of sneezing is obviously for the purpose of removing any irritation from these passages, and by means of the interesting observations of Mr. C. Bell, to which I have so frequently referred, we are able to trace the nervous communications which connect the mucous membrane of the nose, with the muscles that are concerned in respiration,⁸ but there is still some difficulty in explaining the physical causes of coughing and sneezing as distinguished from each other. Laughing is produced by an inspiration succeeded by a succession of short imperfect expirations. Although it may be produced by certain bodily sensations, yet for the most part, it depends upon a mental emotion; the theory of laughter, or the connexion which there is between the action and the causes which excite it, is somewhat obscure. The action of weeping is very similar to that of laughing, although its causes, both corporeal and mental, are so dissimilar. It consists in an inspiration, which is succeeded by a succession of imperfect expirations. Both laughter and weeping are supposed by many physiologists to be confined to the human species; but we

⁸ See particularly the observation in *Phil. Trans.* for 1822, p. 287. *et seq.*

may observe approaches to them in some of the animals which, in other respects, exhibit the most intelligence and sagacity. Hiccup is a quick, involuntary, convulsive contraction of the diaphragm, occurring at intervals, and produced by irritation of the cardiac extremity of the stomach, the gullet, or other neighbouring part. The only remaining action which is connected with the respiratory organs is vomiting, but as this involves some physiological considerations, which are connected with the functions of the digestive organs, it will be more properly considered in a subsequent part of the work.

§ 7. *Of Transpiration.*

As the functions of the skin, and the action which it exercises over the animal œconomy, are generally supposed to bear a considerable analogy to those of the lungs, it may be convenient to introduce in this place an account of the cutaneous transpiration. It seems to have been a very early opinion among physiologists, that besides the visible matter of perspiration, a species of invisible vapour is likewise discharged from the surface of the body, and that this discharge is connected with some of the most important operations of the system.⁹ The first person who endeavoured to ascertain the amount of this vapour, or, indeed, who may be said to have adduced any very unequivocal proofs of its existence, was Sancto-rius.¹ He devoted his almost undivided attention to this subject for the greatest part of his life, and al-

⁹ Haller, *El. Phys.* xii. 2. 4.

¹ *Ibid*, xii. 2. 10.

though we shall probably be inclined to think, that the information which he obtained from his researches was, by no means, proportionate to the labour which he bestowed upon them, yet so pre-eminent was he above his contemporaries, as an inquirer into the operations of the living body, by the mode of experiment, that he obtained the highest degree of celebrity.

The method which Sanctorius adopted to measure the quantity of the insensible perspiration, as he termed it, was to notice accurately the food that was received into the body, and all the discharges that proceeded from it; the former of these quantities was found, in all cases, very considerably to exceed the latter, and this excess was supposed to be transpired from the skin, in the state of invisible vapour. By comparing the weight of the body under all the circumstances to which it is exposed, or by which its functions are modified, as well in health as in disease, a due allowance being always made for the proportion of the ingesta to the egesta, he endeavoured to ascertain the amount of the insensible perspiration during these different states, and he deduced from them a series of aphorisms, which were supposed to contain the general deductions from his almost innumerable experiments.² It is, however, not a little remarkable, and certainly much to be regretted, that he no where gives us any exact numerical account of his results,

² *Medicina Statica*; this celebrated work appears to have been published in the year 1614, at Venice, "unde," as Haller observes, "innumerabiles editiones prodierunt."

and from his having, in most instances, blended his theories with his experiments, and admitted a certain share of hypothesis into his conclusions, there are comparatively but few of them on which we can place much confidence, or at least, which we can adopt without making many exceptions and allowances.

The example of Sanctorius was followed by several other physiologists, who, partly, in consequence of their being aware of his deficiencies, and partly, from the improvements which took place in the mode of conducting philosophical inquiries, employed a more satisfactory method both of executing and of detailing their experiments, many of which appear to have been pursued with much assiduity, and recorded with great accuracy.³ Among those who seem to have been the most successful in their investigations on this subject, we may enumerate Dodart,⁴ Keill,⁵ Rye,⁶ Gorter,⁷ Linings,⁸ Robinson,⁹ Home,¹ and Stark,²

³ We shall find in Haller, *El. Phys.* xii. 2. 10, 12, 13, an account of the successive sets of experiments which were performed on this subject, detailed with his usual minuteness and correctness.

⁴ In *Mem. Acad.* t. ii. p. 276. there is a notice of these experiments.

⁵ *Medicina Statica Britannica*, a small treatise appended to his "Testamina."

⁶ *Medicina Statica Hibernica*, appended to Rogers on epidemic diseases.

⁷ Gorter, *de Perspir. insens.* cap. 2.

⁸ Linings's observations were made at Charlestown, and contain the result of a year's experiments performed on his own person; *Phil. Trans.* for 1743, p. 491, et seq. and for 1745, p. 318, et seq.

all of whom performed experiments, more or less resembling those of Sanctorius, and greatly added to their value by generalizing the results, or reducing them into the form of tables, exhibiting the loss which the body is supposed to sustain by its insensible perspirations in various constitutions, ages, and temperaments, at different periods of the day and seasons of the year, after exercise and repose, after fasting and taking food, sleep and watching, and various other circumstances of an analogous nature. They proceeded upon the plan of Sanctorius, of weighing the body, adding the aliment received, deducting the discharges, and placing the loss which it had sustained to the account of the cutaneous transpiration. Without impeaching the accuracy of the operator, we should expect that experiments performed upon different individuals, and under such a variety of circumstances, must afford very various results, and accordingly, the quantities obtained are so different from each other, that it seems almost

9 "Dissertation on the food and discharges of human bodies;" a learned treatise, proceeding upon the principles of the mechanical physiology, containing however many important observations both original and collected.

¹ Medical Facts, p. 235.. 253.

² Works, p. 169. 182. Stark's experiments, there is every reason to believe, were executed with the most scrupulous accuracy, and afford us many valuable results, but as they were performed principally with a view to ascertain the comparative effect of different kinds of diet, upon the cutaneous transpiration, they are not adapted to the purpose of comparison with the other experiments that are referred to above: his general conclusions are stated in p. 178.

impossible to draw from them any satisfactory average. If I were to select any of these more early experiments, as having been apparently conducted in the most judicious manner, and with the greatest accuracy, it would be those of Rye; his general result is, that the excess of the weight of the ingesta over the egesta, that of the body remaining the same, is 57 ounces in 24 hours.³

But besides other sources of inaccuracy, depending either upon the mode in which the experiments were performed, or the false hypothesis with which they were frequently implicated, there was one fundamental error which pervaded the whole, that the action of the lungs was necessarily confounded with that of the skin. Although it was known that a quantity of aqueous vapour is discharged along with the air of expiration, yet it seems to have been, in a great measure, disregarded in all the calculations, and no one attempted to estimate its amount, with any degree of accuracy, before the time of Hales. I have already had occasion to notice the result of Hales's experiments,⁴ and of the others which were

³ He found the greatest loss of weight in the summer months to be 93 ounces, the least 33, giving an average of 63 ounces; the greatest loss in the winter months was 60, the least 42, giving an average of 51, or of 57 for the whole year. Rogers, p. 310. Aphor. 99. Linings, whose experiments were made in a much warmer climate, found the proportion of the summer to the winter perspiration to be in the proportion of 2.06 to 1, in each case taking the average of the same length of time, 30 days; Phil. Trans. for 1743. p. 509.

⁴ P. 106.

afterwards instituted for the same purpose, which showed that the quantity of water expired from the lungs is so considerable, as to induce Haller to conclude, that in order to compensate for this, and for some other excretions which had been neglected in the calculations, one-half of the estimated quantity of the insensible perspiration should be deducted, which would reduce the average to about 28 ounces in the 24 hours.⁵ This, however, must be regarded as a very vague estimate, that is imperfect in all its essential points; for not only was the mode employed to ascertain the quantity of water expired altogether inadequate to the purpose, but the other effects which the lungs produce upon the air were, at that time, entirely unknown. We have, however, found that one of the most important of these effects consists in the abstraction of a portion of carbon from the blood, which, as well as the aqueous vapour, must, according to the mode in which the experiments were conducted, have been necessarily confounded with the cutaneous transpiration.

After the time of Haller we have no experiments on the cutaneous transpiration, which can be regarded as particularly deserving our attention, until the celebrated ones, to which I have already had occasion to refer,⁶ that were performed by Lavoisier and Seguin.⁷ As these appear to have been executed with great dexterity, and with an apparatus much superior to any which had been hitherto employed in physiolo-

⁵ *El. Phys.* xii. 2. 11.

⁶ *P.* 75, 6.

⁷ *Mem. Acad. Scienc.* pour 1790. p. 601.

gical researches, so the conclusions to which they lead are proportionally important, and although they will be found to be not unobjectionable, yet they very materially advanced our knowledge of the animal œconomy, at the same time that they gave a fresh impulse to the progress of inquiry, and opened a new path for future investigations.

The authors commence by observing, that transpiration consists in the evaporation of a quantity of water from the body, part of which proceeds from the pores of the skin, and part from the inner surface of the vesicles of the lungs, forming respectively the cutaneous and the pulmonary transpiration. Hence there are three distinct operations going forwards in the living system, which affect the weight of the body, and which have been generally confounded together in the statical experiments that have been performed upon it, but which it is necessary to distinguish from each other,—the cutaneous transpiration, the pulmonary transpiration, and the respiration. In order to separate these effects from each other, the body was enclosed in a silk bag, varnished with elastic gum, so as to render it air-tight, while a tube was adapted to the mouth of the operator, by which means it was conceived that the effects of transpiration and of respiration would be kept distinct from each other. This, however, would not be accomplished by the arrangements of the apparatus; they would preserve the cutaneous transpiration distinct from the respiration, but this last would not be kept separate from the pulmonary

transpiration. In performing the experiments the body was weighed before entering the apparatus, and again just before leaving it, for the purpose of finding what weight is lost by respiration as distinct from transpiration; it was again weighed immediately after leaving the apparatus, by which the total loss of weight was obtained, and the former quantity was subtracted from the latter, in order to discover the loss by transpiration alone. But it is obvious that, upon this plan, the effects of the pulmonary transpiration and of the respiration are confounded together, nor are there any direct experiments by which the quantity of the pulmonary transpiration was attempted to be ascertained.

In giving an account of Lavoisier's opinion respecting the change which the blood experiences by respiration, I have mentioned that he conceived the water which is expired from the lungs to be, in part at least, actually generated in that organ by the union of oxygen and hydrogen; and in examining into the nature of the pulmonary transpiration he takes occasion to recur to the same hypothesis. He remarks that a substance which is chiefly composed of hydrogen and carbon, is secreted from the blood, and transudes through the membranes of the lungs into the bronchia; that while it is passing through the exhalent vessels in an attenuated state, it is brought into contact with the oxygen contained in the air of inspiration, with which it unites and is converted into water and carbonic acid. Besides the water which is thus produced in the lungs, a quantity is supposed

to transude through the membranes of the vesicles ready formed; they are both of them reduced into vapour by the warmth of the part, and compose the pulmonary transpiration. A calculation is then entered upon for the purpose of determining the quantity of water which is transpired by the lungs, but it proceeds upon data which I conceive are not correct. The authors lay it down as the basis of their reasoning, that the whole of the oxygen which is consumed in respiration is united to hydrogen and carbon which it meets in the lungs, whereas I have endeavoured to show, that the union does not take place in the lungs, that a part of the oxygen is absorbed and appropriated to some other purpose, and that no combination of oxygen and hydrogen takes place. It is obvious that if any part of the above statement be found to be erroneous, it must vitiate the whole of the reasoning, so far at least as the quantity is concerned; so that although the results of the experiments are valuable as matters of fact, we are unable to follow the authors in the conclusions which they deduce from them.

With respect to the estimates, we are informed that the average quantity carried off by the cutaneous transpiration in 24 hours is 30 ounces, while that by respiration (including as it appears the pulmonary transpiration) is 15 ounces. Of these 15 ounces, rather more than $\frac{1}{3}$ is supposed to consist of the water which is transuded through the lungs, properly constituting the pulmonary transpiration, the other $\frac{2}{3}$ consisting of the hydrogen and carbon, which is

supposed to combine with oxygen and form water and carbonic acid, the quantity of the water being determined by that of the oxygen which is left after subtracting what is required for the formation of the carbonic acid. It appears, however, from the remarks that were made in a former section, that this hypothesis of the generation of water in the lungs is without foundation; so far, therefore, as the existence of this portion of water rests upon that hypothesis, it must be considered as equally unfounded.

There does not appear to be any theoretical objection to the method that was employed to ascertain the amount of the cutaneous perspiration, yet I conceive that many circumstances may occur, which would practically interfere with the results, so that I think we can only consider it as an approximation, and that perhaps not a very close one, to the truth. The near coincidence between the quantity which Lavoisier and Seguin obtained, and the average formed by Haller, may appear remarkable, yet, I apprehend, that it is to be regarded as purely incidental, and not as, in any degree, tending to confirm the accuracy of the estimate. In the present state of our information, the only method that we seem to have of estimating the pulmonary transpiration, is to take the total quantity lost by the body after making the due allowance for the excess of the ingesta above the egesta; from this we must deduct the loss by the cutaneous transpiration, and also the carbon which is emitted from the lungs; what remains may be supposed to be due

to the pulmonary transpiration ; but it is obvious that we can place very little dependence upon a calculation founded on such uncertain and inadequate data. The points which seem to be ascertained are, that the body loses a certain quantity of weight, besides what can be attributed to any of the visible discharges and to the carbon and water which is emitted from the lungs, that a certain quantity of vapour is emitted from the skin, and it may be inferred that this is the principal source of the loss which is experienced. Lavoisier and Seguin do not expressly state what is their opinion respecting the origin of the matter of transpiration, but it would appear that they considered it as arising from a fluid which is secreted by the cutaneous vessels from the blood, and is converted into vapour by the caloric which it abstracts from the body.

We have some very interesting observations on transpiration by Dr. Edwards, the principal object of which is to illustrate the effect produced upon this function by the various circumstances to which the body is subjected. He began by a series of experiments on cold-blooded animals, as we have here the advantage of being easily able to obtain the cutaneous transpiration entirely distinct from the pulmonary, in consequence of the length of time which these animals can live without respiring ; in this way he not only very unequivocally proved the existence of the transpiration by the skin, but ascertained with more certainty its comparative quantity in the different circumstances referred to above. In this case

nothing more was necessary than to weigh the animal before and after the experiment, and to make allowance for the ingesta and the egesta, there being no other organ except the skin, by which any thing is removed which can affect the weight. Some of his most important results are, that the body successively loses less and less by transpiration in equal successive portions of time, depending, as we may presume, upon the absolute quantity of fluid in the vessels; that the transpiration proceeds more rapidly in dry than in moist air, in the extreme states, nearly in the proportion of 10 to 1; that temperature has also a great effect, the transpiration at 68° (20° C.) being twice as much, and at 104° (40° C.) seven times as much as at 32° . He also found that frogs transpire while they are in water, as is proved both by the diminution which they experience while immersed in water, and by the appearance of the water itself, which becomes visibly impregnated with the substance that is excreted from the skin.⁸

Dr. Edwards then extended his researches to the warm-blooded animals. He found that in these, like the others, the transpiration became less in proportion to the quantity of fluid which was evaporated from the body; he also observed the same kind of difference between the effect of moist and dry air, and between a high and a low temperature. The experiments were performed on guinea-pigs and on birds; and it appeared that in all the essential particulars these two kinds of animals agreed in the mode of

⁸ De l'Influence, &c. part. I. C. 5, 6.

their transpiration as affected by external agents.⁹ The transpiration of man was likewise found subject to the same general laws, but probably, in consequence of the greater delicacy of the human frame, and the more complicated nature of his functions, it is more subject to be disturbed by external agents. A circumstance is noticed by Dr. Edwards, which had not been sufficiently attended to by his predecessors, that in endeavouring to ascertain the comparative amount of the transpiration under different circumstances, it is necessary to take intervals of considerable length, in order that the results may not be influenced by the effect of the fluctuations which are always occurring, and which constituted one principal source of the apparent irregularities, that produced so many anomalies in the older experiments. A period of six hours was thought to be necessary, and this he accordingly employed in all his researches. With respect to the different states of the air, its effects upon the cutaneous transpiration were essentially the same on man as on other animals; among other observations he found that the transpiration was more copious during the early than the latter part of the day, that it is greater after taking food, and although most of the vital functions appear to be diminished during sleep, it appeared upon the whole, that the transpiration was increased during this state.¹

⁹ Ibid. part 3. C. 7.

¹ De l'Influence, &c. part. 4. C. 11. Where the data are confessedly so insufficient, it may appear to little purpose to found

The author next proceeds to investigate the nature and source of the matter which is transpired. He

any calculation upon them; still it may be desirable to form the best estimate that we have it in our power to deduce from them. The numbers will be as follows :

Ingesta in 24 hours.		oz.
Food, according to Rye.....		96
Oxygen retained in the system, vide supra, p. 111.		4
		<hr/>
		100
Egesta in 24 hours.		
Urine, according to Rye, }	Rogers, p. 310, l.	{ 40
Alvine discharge, Do. }		
Various other excretions, see Haller, El. Phys. xii. 2. 11.		3
Carbon discharged from the lungs, vide supra, p. 111.		11
Water expired, according to Menzies, vide supra, p. 107.		6
		<hr/>
		66

The ingesta will therefore exceed the visible egesta by 34 oz. which may therefore be assumed as the average amount of the transpiration in 24 hours. Perhaps, a farther addition should be made to this quantity, in order to compensate for the water which is absorbed by the skin; for, as I shall have occasion to observe hereafter, it is probable that an action of this kind takes place, although it appears quite impossible to ascertain its amount. An objection, which is not without considerable weight, has been urged against the supposition of so large a quantity of water being discharged from the skin, that many nations are in the habit of smearing the surface of the body with substances, which it is supposed must prevent any thing from being discharged from it, Haller, El. Phys. xii. 2. 19; and many individuals fall under our observation, in whom, from various accidental circumstances, the skin is frequently so covered with extraneous substances, as apparently to obstruct any discharge from its pores. The only method of obviating this difficulty is to suppose that the pulmonary transpiration is in this case proportionally increased to supply the deficiency.

begins by making a distinction between what is carried off from the body by evaporation, and what is removed from it by transudation; the first depending upon a mere physical operation, in which a substance is converted into vapour, by the addition of heat, while transudation is a vital process, of the nature of secretion or excretion. He observes that the terms evaporation and transudation are not synonymous with the insensible and sensible perspiration respectively of the older writers, because a part of what is removed by transpiration is first transuded, and then evaporated. Evaporation may take place from the dead body, while transudation can only take place from the living body; transpiration is, therefore, properly an operation of an intermediate kind, where the fluid is furnished by a vital function, while it is removed from the body by a mere physical process. The older physiologists were much divided respecting the question, whether the matter of the sensible and the insensible perspiration were originally the same substance, the former being in the fluid state, the latter in the form of vapour. Haller was inclined to suppose that they were essentially different,² and Dr. Edwards appears to be of the same opinion, although it is not very clear, whether he considers that the whole, or only a part of the matter

I am not aware, however, that any experiments were ever performed on these individuals, and until this be done, it would be improper to speculate upon the subject.

² *El. Phys.* xii. 2, 9.

of the insensible transpiration is not derived from the sensible transpiration.³

The two operations of evaporation and transudation being considered as, to a certain extent, independent of each other, it became an interesting object to endeavour to ascertain the degree in which they are each of them respectively exercised. For this purpose Dr. Edwards had recourse to cold-blooded animals, in which we can easily suppress the evaporation, by placing them in air saturated with moisture, and which will of course be nearly of the same temperature with themselves; in this case therefore we can obtain the loss by transpiration alone. By performing this experiment on frogs at a medium temperature not exceeding 68° (20 C.) the evaporation was found to be to the transpiration as 6 to 1; and as the transudation in this animal is very copious, we may infer that in man the proportional quantity of the evaporation is still more considerable.⁴ With respect to the comparative action of the skin and the lungs, it is supposed that what is lost by the lungs must be entirely due to evaporation, as nothing can be removed from them except what is carried off in the state of vapour, mixed with, or dissolved in the air of expiration, so that strictly speaking we have no pulmonary transudation. On this account we may presume that the loss by the skin will be greater than that by the lungs, although it must be expected that the former will be much more variable.

³ De l'Influence, &c. § 6. p. 331. et seq.

⁴ Ibid. p. 334. et seq.

The distinction upon which Dr. Edwards so much insists between evaporation and transudation, although one of great importance, had been but little attended to by preceding physiologists. But on some occasions, I conceive it has been carried by him too far, as where it is maintained that the lungs can transpire only by means of evaporation and not by transudation, and that, in this respect, their action differs from that of the surface of the body. I should suppose, on the contrary, that in both cases, the matter to be transpired must leave the mouths of the vessels in the fluid form, and that the fluid is subsequently evaporated by means of the air, so that, except in degree, I do not perceive any essential difference between the operations.

There is another function that is nearly allied to the one which we have been now considering, and which has been made the subject of experiment by some of the modern physiologists; the chemical action which the skin has been supposed to exercise upon the air contiguous to it. Shortly after the discovery of the production of carbonic acid by the lungs, an inquiry was instituted whether the same kind of change was effected at the surface of the body; a circumstance which seemed in itself not improbable, the only difference between the relative situation of the air and the blood, in the lungs and in the skin, being the different thickness of the membrane by which they are separated. The first experiments on this subject appear to have been those of Millet, who, while the body was immersed in the

warm-bath, observed a number of minute air-bubbles attached to it, some of which he collected, and upon their being examined by Lavoisier, they were found to consist of carbonic acid.⁵ Mr. Cruickshanks and Mr. Abernethy afterwards analyzed the air in which the hand or foot had been confined for a certain length of time, and detected in it a considerable quantity of carbonic acid, thus obviating an objection that has been urged against the experiments of Millet and some others of a similar kind,⁶ that the carbonic acid proceeded, not from the skin but from the water, in which it had been dissolved, and from which it was mechanically separated merely by the presence of a solid substance to which it could attach itself; and accordingly when the experiment was repeated by Priestley,⁷ and due care taken to prevent the adhesion of the small air-bubbles to the body, the production of carbonic acid did not take place.⁸

⁵ Mem. Acad. Science, pour 1777. p. 221, 360.

⁶ Cruickshanks on Insens. Persp. p. 81, 2.; Ingenhourz, sur les Veget. sect. 28. t. i. p. 131. et seq.; Abernethy's Essays, part 2. p. 115. et seq.

⁷ On Air, v. ii. p. 193, 4.

⁸ We have a series of experiments on this subject by Troussel, which seem to have been performed with accuracy, accompanied with remarks on the opinions of his predecessors; his conclusion was that oxygen is consumed and nitrogen only left, without the production of carbonic acid; Ann. Chem. t. xlv. p. 73. et seq.; Nicholson's Jour. v. v. p. 50. et seq. It appears, however, that this result has not been confirmed, and it does not accord with any hypothesis that we can form upon the subject; if the skin possesses the power of attracting oxygen, it is highly probable that it will produce carbonic acid.

A set of very elaborate experiments were performed by Jurine on the effect which the skin produces upon the air, the results of which appeared to prove very decidedly, that oxygen is consumed and carbonic acid generated in the same manner as in the lungs. He not only established the general fact, but he examined the quantity of effect which is produced under the various circumstances to which the body is exposed, either as influenced by external agents, or as connected with the different states of the constitution; and it seemed to be proved that the amount of carbonic acid was in exact proportion to the activity of the circulation, and the other functions dependent upon it.² Jurine's experiments were of so simple and direct a nature, that it is not very easy to conceive how he could have fallen into any material error either in their execution, or in the inference which he deduced from them; yet we have, on the other hand, the accounts of other physiologists, who obtained totally opposite results. Priestley was never able to detect the smallest portion of carbonic acid in air that had been kept in contact with the skin;¹ the same was the case with Dr. Klapp,³ and Dr. Gordon,³ both of whom seem to have conducted the process with every requisite attention to accuracy, but could never perceive the least effect to be produced by it.

² Mem. Med. Soc. t. x. p. 53..72. and Encyc. Meth. "Medecine," t. i. p. 510., 515; this volume was published in 1787.

¹ On Air, v. v. p. 100..7. (1st sect.) v. ii. p. 192..9.

² Ellis's Inquiry, p. 189, 0. and 354, 5.

³ Ibid. p. 355, 6.

Mr. Ellis, however, informs us, that he was present when the experiment was made by Dr. M'Kenzie, and reports that, in this case, there was an evident production of carbonic acid.⁴

So far, therefore, as the result of experiment is concerned, it appears difficult to decide upon the question respecting the chemical action of the skin on the air in the human subject, the authorities on each side being, as we find, so nearly balanced. But in the cold-blooded animals, where, from various causes, it is more easy to perform the experiment in an unexceptionable manner, it is admitted by every one that the skin possesses the power of acting upon the air. In many of the lower tribes the lungs are entirely wanting, yet they consume oxygen and generate carbonic acid like the more perfect animals, and in the oviparous quadrupeds, who are furnished with lungs, it appears that the effects produced upon the air by the external surface of the body, is nearly

⁴ Inquiry, p. 358. Mr. Ellis, after stating the facts that have been brought forwards on each side of the question, endeavours to reconcile the apparent contradiction of evidence, by supposing that oxygen is not absorbed by the skin, nor carbonic acid discharged from it, but that carbon is excreted by the exhalents, which unites with the oxygen of the contiguous air, thus extending to the skin the same action which he conceives to take place in the lungs; Inquiry, p. 358. But it may be remarked upon this hypothesis, that whatever may be the nature of the action of the skin upon the air, the result would be the same as to the change upon the air; and that we are equally unable to explain why Jurine obtained carbonic acid, while Priestley and others could not procure it.

equal to that of the pulmonary cavities.⁵ These considerations have been supposed by some physiologists to afford a proof of the existence of the same action in the higher orders of animals, but when so much difference of structure exists, the analogy seems to be of very doubtful application.

APPENDIX.

In the *Ann. de Chim. et Phys.* for Oct. last, t. xxx. p. 191. 3, there is a notice of a memoir "On the Influence of the Atmosphere on the Circulation of the Blood," by Dr. Barry of Paris, a report of which was presented to the Academy on the 15th August. The author observes, that in the act of inspiration a vacuum is produced in the chest, and that consequently, every fluid which communicates with it, will have a tendency to be drawn into it. A variety of facts are adduced to prove that this is the case, such as the swelling of the jugular veins in inspiration, the cessation of certain hæmorrhages in forced inspiration, and the absorption of air into the veins. A series of experiments was performed to prove this position. They consisted in inserting one end of a tube into the jugular vein, while the other end was immersed in a coloured fluid, when it was

⁵ Spallanzani, *Mem.* p. 150. et seq.; *Rapports*, t. i. passim. Ellis's *Inquiry*, § 142, p. 179; § 662, p. 353. et alibi. Edwards de l'*Influence &c.* part 1. ch. 1. § 4. and ch. 4. *Mem. d'Arcueil*, t. ii. p. 393, 4.

observed that whenever the animal inspired, the fluid ascended in the tube, while during expiration, the fluid remained stationary or was even repelled. The account which we have of the experiments is too concise for us to form any very accurate estimate of their value, but they appear to resemble some that were performed by the early physiologists, for the purpose of proving that the chest is more pervious to the passage of the blood during inspiration than during expiration. The obvious objection to such experiments in the abstract is, that they apply to what occurs in extraordinary states of the respiration, rather than to its ordinary action, and that in the healthy condition of the system, the different states of the thorax cannot be perceived to affect the pulse.

CHAPTER VIII.

OF ANIMAL TEMPERATURE.

ONE of the most remarkable circumstances which distinguishes the living body from dead or inanimate matter, is the power which it possesses of resisting, to a certain extent, the changes of external temperature, or of maintaining a more or less uniform degree of heat, independent of that of the substances with which it is in contact. As animals are, in almost all instances, immersed in a medium that is colder than themselves, we have much more frequent opportunities of observing their power of generating heat than cold; but we shall find that they are capable of producing both these effects, and although it will probably appear that they depend upon essentially different operations, yet being intimately connected with each other, it will be convenient to consider them in the same part of the work.

After noticing the general fact of the power which animals possess of regulating their temperature, it is necessary to observe that the different species differ very materially in the degree in which they possess this power; those that have the greatest number of organized parts, and whose functions are, in other respects, the most perfect and varied, being able to resist the changes of external temperature much more effectually than the lower classes. Hence arises the great division of animals into warm and cold-blooded,

the first including the human species, the mammiferous quadrupeds, and birds, the second, the oviparous quadrupeds, fishes, and, with a few exceptions, all the invertebrated animals. The first of these divisions, the warm-blooded, under ordinary circumstances, retain the temperature of their internal parts at a certain standard, which is nearly uniform for each of the three classes. The temperature of birds is the highest, being about 107° or 108° , that of the viviparous quadrupeds is about 100° or 101° , while the human temperature is a little lower, being 97° or 98° .¹ In

¹ We shall find the statements that are made by different authors of the temperature of warm-blooded animals not to be entirely uniform. This may depend, in some measure, upon the inaccuracy of the instruments, or upon the mode in which they were applied, but we shall also find that the temperature itself is liable to be affected by circumstances of which we were formerly not aware. Martine, who on all subjects respecting temperature, may be regarded as one of the most correct of the earlier writers, has given us a number of observations made by himself and others, from which he deduces the human temperature to be "about 97° or 98° ;" the heat of the warm-blooded quadrupeds and the cetaceæ, he fixes at about 4° or 5° above that of man, and the heat of birds about 4° or 5° still higher; *Essays*, No. 4. Art. 4. p. 143, et seq. Martine's estimate of the human temperature seems to be sanctioned by the most correct of the modern physiologists; it is also generally admitted that the temperature of birds is some degrees higher, although, perhaps, the difference may be less than he imagined. Hunter estimates their temperature at 3° or 4° above the mammalia; *Phil. Trans.* for 1778, p. 23. 5. Richerard says, that their temperature is from 8° to 10° above that of man; *Physiol.* § 79. p. 215. Blumenbach estimates the human temperature at 96° , and that of birds, he remarks, is considerably

the cold-blooded animals the temperature is much less uniform, and indeed nearly follows that of the

higher; *Physiol.* p. 96. Reeve, that it is from 3° to 6° higher than that of quadrupeds; On Torpidity, sect. 3. p. 61; Dr. Thomson that it is 103° or 104° ; *Chemistry*, v. iv. p. 630; Dr. Edwards in one part of his work, p. 136, remarks that their temperature is from 2° to 3° (cent.), and in another passage, p. 158, from 3° to 4° (cent.) greater than that of the mammalia. We meet with a curious observation in Boyle, which, if correct, may bear upon this point, that birds, with the exception of water-fowls, are more easily drowned than other animals; *Works*, v. iii. p. 368.

It is uncertain how far the peculiar structure which is connected with the pulmonary cavities in birds is related to their higher temperature; Hunter, in his essay on the "Air-cells in Birds," judiciously remarks, "I can hardly think that any air which gets beyond the vesiculated lungs themselves is capable of affecting the blood of the animal, as the other cavities into which it enters, whether of the soft parts or of the bones, appear to be very little vascular." *Observ. on the Anim. Œcon.* p. 97. This consideration would lead to the idea, that the use of these cells is rather mechanical than chemical, and is in some way connected with the act of flying. Hunter indeed supposes it to be an objection to this opinion, that those cells are equally found in birds that are incapable of flight; see his paper in *Phil. Trans.* for 1774, p. 205. et seq.; but an objection of this kind might be urged with respect to almost every organ and function with which the body is provided. In an abstract of a paper of Despretz, published in the *Edinburgh Jour. of Scien.* v. iv. p. 185, the temperature of pigeons is said to be 109.371° , that of man being between 93° and 99° , and of a dog 103° .

With respect to the extreme variations of the human temperature, Dumas informs us that it ranges from 87° to 108° ; he fixes the habitual degree at 95° or 96° , *Physiol.* c. 6. t. iii. p. 126. M. Magendie notices the general fact of the variation to which the human temperature is subject, as depending upon constitu-

medium, whether air or water, in which they are immersed, being generally one or two degrees above

tion, temperament, &c.; he agrees with Dr. Edwards in recommending the arm-pit as the most proper situation for applying the thermometer; *Physiol. t. ii. p. 403.* Dr. Edwards, as well as M. Magendie, observes that there is a little difference between the temperature of different individuals; he informs us that he examined 20 persons, and found it to vary from nearly 95° to $98\frac{1}{2}^{\circ}$, (35.5 to 37 cent.) the mean of which will be about 97° . (36.12 cent.) This refers to the adult, for in infancy the temperature is decidedly less, being upon the average about $94\frac{1}{2}^{\circ}$ (34.75 cent.). See also the observations of Despretz, in *Edin. Jour. of Scien. v. iv. p. 185.* He also informs us that it varies according to the season of the year, gradually increasing during the spring and declining again during the autumn; he found the difference in birds to be nearly 4° cent.; from 105° to nearly 111° (from 40.8 to 43.77 cent.) p. 489. It is not very easy to ascertain what is the greatest height to which the human temperature is occasionally raised in fevers or other morbid conditions of the body; as until lately the observations have been seldom made with much accuracy. It would appear from various notices in Currie's Medical Reports, that in the most acute fevers the temperature seldom or ever exceeds 107° . See also *Edin. Med. Jour. v. xxii. p. 363.* Dr. Edwards relates an observation of M. Prevost's, where a patient in tetanus had the heat elevated 7° cent. above the ordinary standard; p. 490. We have a singular statement made by M. Magendie, of the high temperature which the blood occasionally acquires in local inflammation, but the authority is not quoted; *Physiol. t. ii. p. 400.* Hunter, in his work on the *Anim. Econ.* note to page 113, observes, "It is found from experiments, that the heat of an inflamed part is nearly the greatest or standard heat of the animals, it appearing to be a part of the process of inflammation to raise the heat up to the standard;" but there is probably some inaccuracy either in the observation or the expression. In his work on the blood,

it, in the ordinary state of the atmosphere.² The topics which will more particularly require our examination on this subject are, 1. What is the efficient cause or source of animal heat; 2. By what means is its uniformity preserved; 3. How is the body cooled at high temperatures; and lastly, what is the connexion between the functions of calorification and the vital powers of the system.

he gives us the result of a number of observations which he had made upon the temperature of an inflamed part; the greatest heat that is recorded is 104° , p. 296. He remarks that the actual temperature in inflammation is not so much increased as the sensation would seem to indicate. Dr. Granville has lately communicated some curious facts respecting the temperature of the uterine system during parturition, from which it appears that it occasionally rises to 120° , the elevation appearing to bear a proportion to the degree of action in the organ; Phil. Trans. for 1825, p. 262.. 4.

² For the temperature of the cold-blooded animals it may be sufficient to refer to Martine, p. 241 et seq.; Hunter on the Anim. Econ. p. 116, 9; also on the Blood, p. 298 et seq.; Spallanzani, Mem. p. 255 et seq.; and Ellis, p. 215 et seq. Hunter observes that the warm and cold-blooded animals might be more properly termed animals whose heat is permanent or variable, according to that of the atmosphere; On the Blood, note in p. 15; also Phil. Trans. for 1778, p. 26.. 8; where he relates a number of experiments on the degree in which the temperature of the cold-blooded animals is affected by the medium in which they are immersed. Broussonet remarks that the temperature of fish is from $\frac{3}{4}$ to $\frac{1}{2}$ a degree (R.) higher than that of the medium in which they are immersed; Mem. Acad. pour 1785, p. 191, and M. Despretz found the temperature of a carp, immersed in water at 51.4 to be 53° ; Edin. Jour. of Scien. v. iv. p. 185.

§ 1. *Of the efficient cause of Animal Heat.*

The body is surrounded by an atmosphere, which is frequently 40, 50, or even a greater number of degrees colder than itself, so that heat must be rapidly abstracted from it, yet it possesses the power of continually supplying the loss thus occasioned. This faculty appeared to the ancients so far beyond the reach of all physical agency, that they did not even attempt to assign any cause for it, but regarded it as an innate quality of the body, or something essentially connected with life, so that in speculating upon the subject of animal temperature, they thought it necessary to direct their inquiries rather to the modes by which the heat of the body might be reduced to the proper standard, than be maintained above that of the surrounding medium. Without entering into any minute detail of opinions, which can be interesting only from their antiquity, it may be sufficient to remark that Galen and the ancients generally conceived animal heat to be an innate or primary quality of the body, and that it is contemporary with life.³ Its origin or focus was supposed to be in the heart, from which, by means of the blood, it was distributed to all parts of the system. A principal office of the circulation was therefore supposed to be the convey-

³ For the opinions of the ancients see Boerhaave, *Prælect.* § 169, 202. cum notis; Haller, *El. Phys.* vi. §. 8; we have an account of the doctrine maintained by Hippocrates on this subject in his *Treatise de Corde*, Op. t. i. p. 269; also in Le Clerc, *Hist. de la Medecine*, p. 125.

ance of this heat to the various organs, while one great use of the respiration was to cool the blood, or to prevent its heat from exceeding the degree which was consistent with the well being of the animal. After the revival of letters, when the causes of the phenomena of the living body began to form an object of investigation, physiologists still very generally regarded the blood as the source of animal heat, and according to the peculiar theory which they had adopted respecting the operations of the system, they explained the production of heat either upon chemical or mechanical principles. The chemists⁴

⁴ See Haller, *El. Phys.* vi. 3. 8, 10, for a summary of these opinions. As a specimen of the opinions and mode of reasoning that were adopted on this subject by the chemical physiologists, I may refer to the Treatise of Willis, "*de Accensione Sanguinis*," which was probably written in the year 1670. The author supposes that there is a proper combustion in the blood, which, according to his general principles, depends upon the fermentation excited by the combination between different chemical substances. He strenuously maintains the doctrine of the life of the blood, which he conceives to consist in its property of producing heat. In the middle of the last century we had a very learned dissertation by Stevenson; *Ed. Med. Essays*, v. v. pt. 2. p. 806, et seq. He observes that four opinions were then prevalent; 1. That animal heat depends upon attrition between the arteries and the blood; 2. That the lungs are the fountain of this heat; 3. That the attrition of the parts of the solids upon each other produce it; and lastly, the process by which our aliments and juices are constantly undergoing some alteration. With respect to the idea of the heat being derived from the lungs, the author adduces various arguments to prove that the blood is rather cooled than warmed in passing through the pulmonary vessels. This final conclusion is in favour of the

ascribed it to fermentation in the blood which took place in the heart, while the mechanicians accounted for it by the friction of the particles of the blood against the sides of the vessels, and consequently, supposed that it was produced during the course of the circulation.⁵

Although we meet with occasional expressions, which may appear to indicate a more correct opinion, yet perhaps the first clear intimation of a regular hypothesis to account for the generation of animal heat, upon what we should now consider as a legitimate mode of reasoning, is to be met with in the writings of Mayow. After he had made his interesting discoveries on the constitution of the atmosphere, and the share which it has in the extrication of heat in combustion, he was led to extend the analogy to animal heat, and concluded, contrary to the opinion almost universally embraced at that time,⁶ that the

last supposition; he remarks concerning it, that heat is frequently excited by the chemical change which results from the mixture of fluids with each other, and that these changes proceed either from fermentation or putrefaction; the case in question he decides to be one of fermentation, or rather something intermediate between the two. He supposes that the process is principally carried on in the veins, and less in the lungs than in the other parts of the body.

⁵ For specimens of the mechanical mode of reasoning, see Boerhaave, *Aphor. cum notis Sweiten*; § 382. 675. Perhaps the last attempt to form a mechanical theory of animal heat (at least in this country) is that of Douglas, published in 1747; he lays down the theorem that, "animal heat is generated by the friction of the globules of blood in the extreme capillaries," p. 47.

⁶ The following are among the eminent physiologists of

use of the lungs is not to cool the heart, but to generate heat, an effect which is brought about by the absorption of the nitro-aereal spirit of the air. This, mixing with the sulphureous particles of the blood, excites a species of fermentation by which heat is produced, in a way precisely similar to that in which heat is excited by the combustion of inflammable matter generally.⁶ Mayow therefore explicitly advanced the two positions, that the effect of respiration is not to cool the blood, but to generate heat, and that it does this by an operation in every respect analogous to combustion. His hypothesis was however imperfect in consequence of the erroneous opinions which he entertained respecting the nature of combustion generally, or the mode by which combustion generates heat, as well as respecting the nature of the combustible matter which is discharged from the blood. Perhaps, however, this latter should be regarded as a verbal, rather than as a real inaccuracy, the word sulphureous being a general term applied to any

that period, who supposed that the effect of respiration is to cool the blood. Sylvius, *Disp. Med.* cap. 7; Fabricius de *Respiratione*, lib. 1. cap. 6; Bartholine, *Anat.* p. 430; Harvey de *Motu Cordis*, Ex. 2. p. 194, 5. and Ex. 3. p. 232; Swammerdam, de *Respiratione*, sect. 1. c. 1. § 9. and c. 3. § 4; Descartes supposes that the fermentation of the blood in the heart produces heat, which is carried from this organ over the body; De *Homine*, p. 197. Boyle remarks that "divers of the new philosophers, Cartesians, and others, think the chief, if not the sole use of respiration, to be the cooling and tempering of the heat in the heart and blood, which otherwise would be immoderate." *Works*, v. i. p. 103. Haller discusses the question in *El. Phys.* viii. 5, 6.

⁶ *Tract.* p. 151 et seq.; 296, 7.

kind of inflammable matter, not as is now the case, confined to one species. Mayow's doctrine respecting the connexion between respiration and animal heat does not appear to have made any considerable impression upon the minds of the contemporary physiologists, for we find that the old hypotheses still continued to prevail with little alteration, or only with slight and immaterial modifications, until the middle of the last century. During this period the question was warmly discussed, whether the lungs were the agents for generating heat, or for reducing the temperature of the blood by bringing it into proximity with the cold air, and the sentiments of the most eminent physiologists appear to have been, for the most part, in favour of the latter opinion. And it must be remarked, that even most of those who adopted the former opinion, that the lungs serve to generate heat, supposed that the effect was produced entirely by friction or some other mechanical means.⁷

It was not until after Black had completed his valuable train of experiments on fixed air, that more

⁷ See Boerhaave, *Prælect.* § 202, 220. cum notis; Boerhaave, *Aphor.* 382. cum *Comment.* Sweiten; Haller, *prim. lin.* § 303; Martine and Stevenson *ubi supra*; Cullen displays his accustomed caution in examining into the cause of animal heat, *Physiol.* § 262; he perceived the connexion which there seemed to be between the lungs and the production of heat, but he could not account for the method in which they act in producing it. Haller, as usual, gives us an account of the opinions that had been entertained on both sides of the question, and inclines to the negative; *El. Phys.* vi. 3. 13. sub finem. Perhaps Morozzo is the latest author of any considerable respectability, who defends the opinion that the effect of respiration is to cool the blood; *Jour. Phys.* t. xxv. p. 120.

correct ideas began to prevail respecting the cause of animal heat. After he had discovered that the same kind of aeriform fluid which is produced by the burning of fuel is expired from the lungs, a strict connexion seemed to be established between the processes of combustion and respiration. He was hence led to infer, as Mayow had previously done, that respiration is a species of combustion, and that the heat thus extricated is employed in preserving the temperature of the animal body above that of the surrounding medium. But Black's explanation of the cause of animal heat, although extremely ingenious, and founded upon what appeared to be correct principles, was liable to an objection which prevented it from being generally received. It was said, that if the lungs are the seat of the supposed combustion, or the focus whence the heat radiates to all parts of the body, their temperature must be much superior to that of the other organs, and in short, that they would be altogether incapable of supporting the degree of heat, to which they would be necessarily exposed. We do not find that Black made any attempt to repel the objection, and we may presume that he conceived it so formidable, as to have induced him altogether to relinquish the hypothesis.⁸

⁸ Although there appears to be no doubt that Black applied his discovery of the formation of carbonic acid in respiration to explain the production of animal heat, we do not find any reference to it in his lectures, as they were afterwards published by Robison. We may, however, conclude that he announced the hypothesis in his lectures, and that it became generally known through this medium; see the Remarks of Menzies on Respiration, p. 35; Murray, Chem. v. iv. p. 571; Thomson, Chem.

Lavoisier now entered upon his researches into the nature of combustion and other analogous ope-

v. iv. p. 630; and Ellis, *Inquiry*, p. 234; Mr. Ellis says, that the objection referred to in the text was urged by Cullen; also Robison in his preface, p. 53. Probably Cullen, in the following passage in his "*Institutions*," § 268. may refer to Black's hypothesis. "We take no notice of the suppositions which have been made of the generating powers being confined to certain small portions of the system only. These suppositions give no relief in the general theory: and they are not supported by any particular evidence. The breathing animals are the warmest; but that they are warmer because they breathe, is not more probable, than that they breathe because they are warmer." Leslie's *Treatise on Animal Heat* contains an account of the popular objections to Black's doctrine; his own hypothesis is that phlogiston is naturally developed by the vessels and generates heat. Dr. Black, with the modesty and scrupulous regard to the literary rights of his contemporaries, which formed so distinguished a trait in his character, when speaking of Crawford's hypothesis of the different capacities of bodies, says, that he applied it "to explain the heat maintained in the bodies of animals," without any allusion to his own opinions; *Lect.* v. 2. p. 205, 6. It was about this period of the investigation that Franklin threw out a conjecture on the source of animal heat, that the matter of heat was taken into the body along with the food, and was set at liberty during the successive changes which the aliment afterwards experiences; *Works*, v. ii. p. 79, 125. This hypothesis was subsequently taken up by Rigby and dilated into a *Treatise*; see sect. 1. of his *Work on Animal Heat*. Richerand, to a certain extent, adopts the same view of the subject, but his remarks are vague and diffuse; *Physiol.* § 79. p. 214. I may remark, that an opinion something similar to this had been previously formed by Descartes; he says that the change which the food undergoes in the stomach produces heat, in the same manner as when water is poured upon lime, or aqua fortis upon metals; *De Homine*, p. 7. Hunter also

rations, and after repeating and verifying many of the experiments of Black and Priestley, he pointed out more clearly than they had done, the exact nature of the change which is produced upon the air by the action of the lungs. He found a perfect similarity between the results which he obtained from the combustion of charcoal and the products of respiration, and as the evolution of heat is the necessary consequence of the former operation, he was induced to draw the same inference that had been previously made by Black, that heat must be generated by the formation of carbonic acid in the lungs. He does not appear, at least in the first instance, to have felt the objection that had been urged against the doctrine, but brings it forwards as a correct deduction from acknowledged facts, and as a satisfactory method of explaining the phenomena.⁹

considers it probable that the principal source of heat is in the stomach; On the Blood, p. 292; the remark is, however, made in an incidental manner, and he had expressed himself in another passage in the same work dissatisfied with all the theories of animal heat, as not according with the facts; p. 15.

⁹ Mem. Acad. Scien. pour 1777, p. 599. In tracing the progress of discovery on this subject, it appears somewhat difficult to ascertain the share which Black and Lavoisier respectively bore in establishing the chemical theory of animal heat. Lavoisier, in the above memoir, speaks of the hypothesis as entirely original, and altogether derived from his own experiments, without referring to any preceding authors, and the same statement is made by Seguin; Ann. Chem. t. v. p. 259; see also Fourcroy, Med. Eclair. t. i. p. 56. 61. There seems, however, to be no doubt, that as far as respects the production of carbonic acid in the lungs, and the consequent evolution of heat, he had been

It was shortly after this period that Crawford commenced his investigations, and illustrated the subject by a series of very elaborate and ingenious experiments; the result of which was the formation of a theory of animal heat, which appeared to account for all the phenomena, and to be founded upon decisive and well established facts, while it was not liable to the objection which had been urged against Black's hypothesis. The scrupulous care with which Crawford endeavoured to avoid every source of inaccuracy, the ingenuousness with which he acknowledged the errors of his first experiments, and the spirit of candour which pervades every part of his work, were calculated to produce a very favourable impression with respect both to the intellectual and the moral qualities of the author, so that his doctrine was very favourably received, and generally embraced by his contemporaries.

Crawford's theory of animal heat may be regarded completely anticipated by Black, while he does not advert to the objection which had been urged against the hypothesis. Although we admit that Robison's zeal for the reputation of his deceased friend may have carried him too far in his observations upon the rival claims of the two philosophers, it seems but too probable that Lavoisier was not always sufficiently correct in appropriating the due share of merit to his contemporaries, a circumstance the more to be lamented, when we consider the very great obligations which he conferred upon science. There are some good observations on this subject in Nicholson's *Jour.* v. xiv. p. 90, 231, et seq. I may remark that Crawford, who was distinguished for his candour and liberality, does not refer to Black, as having advanced any hypothesis of animal heat, nor does he intimate that his own opinions had been anticipated by Lavoisier.

as founded upon the three following positions;

1. That the air, when taken into the lungs, undergoes the same change as by the combustion of a carbonaceous body, and that consequently, in the same manner as in the combustion of carbon, heat is generated. 2. The same process by which the oxygen of the inspired air is converted into carbonic acid likewise converts the venous into arterial blood, but arterial possessing a greater capacity for heat than venous blood, the heat which would otherwise raise the capacity of the arterialized blood, is employed in saturating its increased capacity, and in maintaining its temperature at the same degree with the venous. 3. The heat is not therefore actually set at liberty in the lungs, although the arterial contains a greater quantity of absolute heat than the venous blood, but it is during the course of the circulation, when the arterial blood is again venalized, and consequently loses its increased capacity, that the heat becomes sensible and supports the temperature of the system. The hypothesis which Crawford constructed upon the above positions professed to be the result of direct experiment in all its parts; it removed the objection that had been urged against Black's doctrine, and afforded one of the most interesting and beautiful specimens of the application of physical and chemical reasoning to the animal œconomy that had been ever presented to the world.¹

¹ We have a correct and judicious summary of Crawford's theory given us in Prof. de la Rive's elegant inaugural dissertation "*De Calori Animali*," p. 26, et seq.; and in Dr. Henry's *Elements*, v. ii. p. 407.

The first of the above positions, the different capacities for heat of the air before and after it had undergone the change which it experiences in combustion or in respiration, constituted, as is well known, the main point to which Crawford directed his elaborate train of experiments, and forms the basis of his theory of inflammation generally, of which animal heat composes only one example. With respect to the second position, the different capacities of arterial and venous blood, Crawford made this the subject of a long and careful examination, and although there are many parts in it of very delicate execution, and which required a minute attention to various circumstances that might interfere with the results, yet he appears to have been so well aware of the sources of error, and to have so carefully guarded against them, that, I think, a perusal of the experiments can scarcely fail to impress the mind with a conviction of their general truth. The conclusion which Crawford deduced was, that the specific heat of arterial, was greater than that of venous blood in an average proportion of 114.5 to 100. If we admit the fact, the conclusion is obvious and most important. It follows that when the blood is converted from the venous to the arterial state, it renders latent a part of the heat which would otherwise have been liberated by the union of the oxygen and the carbon, and would have raised the temperature of the blood in the pulmonary vessels. The heat therefore appears to be employed in three ways; a part of it in counteracting the effect of the cold air which is taken into the lungs,

another portion in producing the vapour that is expired, while the remainder supplies the arterial blood with what is requisite, in consequence of its increased capacity, to support its temperature at the same degree with that of the venous blood.

It would appear that these operations are so nicely adjusted to each other, that the temperature of the blood in the different parts of the body is kept nearly at a uniform standard, and especially, that while this fluid passes through the lungs, and in conjunction with the air, undergoes that change which serves as the actual origin of the heat of the animal, its temperature is little, if at all affected, the heat which would otherwise be liberated being the whole of it employed in the three ways mentioned above. We have here a remarkable example of that nice adaptation of the different operations of the living body to each other, which forms so distinguishing a feature of the animal economy, and in which it so infinitely surpasses any contrivances of a merely mechanical or physical nature. Admitting the truth of the theory it will follow, that the more carbon is separated from the blood and united to oxygen, the more heat will be disengaged, the more completely therefore will the blood be arterialized, and consequently the more will its capacity be increased, and the greater quantity of heat will it require to maintain its temperature. According to Crawford's doctrine the blood is not warmed in passing through the lungs, although this is the organ in which it acquires its heat, but it is in the capillary part of the systemic circulation, when

the blood again becomes venalized, that the heat is liberated. This effect therefore takes place in that part of the body where it is the most necessary to counteract the effect of the surrounding medium, which is, in most cases, colder than the body itself, and where by its diffusion over a great extent of surface, it must tend to make the heat nearly uniform through the whole of the system.

After the publication of Crawford's theory, Lavoisier, in a subsequent memoir, recurred to the subject of animal heat. He still maintained his former opinion, that it is derived from the union of carbon and oxygen in the lungs, and that respiration is, in every respect, analogous to the process of combustion. He assumes that all parts of the body are preserved at the same temperature, and with respect to the manner in which the heat is equalized, he observes that it depends upon the three following causes; 1. Upon the rapidity of the circulation of the blood, by which the heat that is acquired in the lungs is quickly transmitted to all parts of the body; 2. Upon the evaporation which takes place from these organs, which carries off a part of their heat; and 3. From the increased capacity for heat, which blood acquires, when it is converted from the venous to the arterial state. It will be observed that this view of the subject, so far as animal temperature alone is concerned, is strictly conformable to the doctrine of Crawford.^a

^a Mem. Acad. Scien. pour 1780, p. 406. We may remark, however, that Lavoisier, in one of his subsequent papers, speaks of the combustion as taking place in the lungs, although he adds,

Lavoisier, however, as is too frequently the case, does not introduce the name of Crawford, or even allude to his experiments on the different capacities of arterial and venous blood, and the same silence is observed on this point, when, in another part of this paper, he compares his own hypothesis with that of Crawford respecting the heat generated by combustion, and notices the experiments on the different capacities of oxygen and carbonic acid.⁴ Lavoisier seems to have maintained the same opinion respecting animal heat in his future researches; he always speaks of it as a case of combustion, and supposes that the heat is generated upon the same principle, as in the formation of carbonic acid by the more rapid union of oxygen and carbon.

Although Crawford's theory so admirably accounted for all the phenomena, and appeared to be so strictly deduced from facts and experiments, yet the extreme delicacy of the operations on which it rested, as well as the interest excited by its great importance, led to a rigid examination of all its fundamental positions; the consequence of which was that they have all of them been controverted, and we have an account of a number of experiments, the results of which are reported as being directly opposite to those of Crawford, or at least, as not warranting his conclusions. The main points on which the theory is supported are the following; that heat is extricated when oxygen is

and perhaps in other parts of the system;' *Mem. Acad. Scien.* pour 1790, p. 601.

⁴ *Mem. Acad. Scien.* pour 1780. p. 394.

converted into carbonic acid, in consequence of the diminished capacity of the latter, that the capacity of arterial is greater than of venous blood, and that the blood in the two sides of the heart, and in the large trunks of the pulmonic system possesses the same temperature. With respect to the first of these points, the different capacities of oxygen and carbonic acid, it must be considered as a question which affects the theory of combustion generally, and in which the cause of animal temperature is not particularly concerned. When carbon is united to oxygen so as to produce carbonic acid, we find that heat is liberated, and this takes place not merely in that rapid union of the bodies, which may be considered essential to a proper combustion, but in the slow combination of them, such as occurs in fermentation, putrefaction, and germination.⁵ In whatever way therefore we account for the liberation of heat in these processes, we may employ the same method to explain the heat generated by respiration. We may even go farther, and say, that as heat is always liberated when this union of oxygen and carbon takes place, the same effects must necessarily follow from the production of carbonic acid in respiration. The difficulty therefore is not to account for the heat produced by respiration, but to remove the objection which was originally urged against Black's hypothesis, that the temperature of the blood in the lungs ought

⁵ In the malting of barley, which is a case of germination, we are informed by Dr. Thomson, that the temperature of the grain is raised as much as 10°; Chem. v. iv. p. 374.

to be greater than that in the other parts of the body. It is in fact with the other two points only, the comparative temperature of the blood in the trunks of the pulmonic vessels or in the two sides of the heart, and the different capacity of arterial and venous blood, that the theory of animal heat is concerned, but on both of these experiments have been adduced which are in direct opposition to the doctrine of Crawford.

It seems not a little remarkable that there should have been any difficulty in ascertaining the first of these points, which we might have supposed would have been settled by a few simple and easy observations. This, however, appears not to be the case, and it still remains undecided, although the weight of authority inclines to the opinion, that arterialized blood, at least in the heart and great trunks, is a degree or two warmer than the venous, so that upon the whole it appears probable that the blood does not possess that uniformity of temperature which has been generally assigned to it, but that it differs a little, not only in different individuals of the same species, but also in the different internal organs of the same individuals.⁶ The latest experiments which

⁶ In estimating the temperature of the blood in the internal parts of the body generally, it was formerly stated that when they are so far below the surface, as to be beyond the reach of the influence of the atmosphere, the heat is uniform; yet the remark must be taken with limitations of which most of the modern physiologists appear to be well aware. Boerhaave observes that the arterial blood is warmer than the venous, although it is exposed to the cold air which is taken in by the

have been made on the comparative capacity of arterial and venous blood are likewise unfavourable to the theory of Crawford, for although we are led to conclude from them, especially from those of Dr. Davy, that the specific heat of arterial is greater than that of venous blood, the excess appears to be so small as to be scarcely sufficient to account for the effects which are attributed to it.⁷

lungs, *Prælect. not. ad.* § 202. The general fact of the different internal parts of the body possessing different temperatures, according to their vicinity to the heart, was established by Hunter, although there may be some reason to doubt whether his experiments were, in all cases, perfectly accurate; *Observations on the Animal Economy*, p. 107, et seq. We have also various observations to the same effect made by Sir A. Carlisle; *Phil. Trans.* for 1805, p. 15, 22. Many eminent physiologists, who have examined the temperature of the blood, since the attention was particularly directed to the point by Crawford's theory, have found the left side of the heart to be warmer than the right. Menzies informs us that this is the fact; *Essay*, p. 58, 62; Plenck says the arterial blood is warmer than the venous; *Hydrol.* p. 33; Dr. Davy found the arterial blood 1° warmer than the venous, and he observes that the parts of the body are less warm as they recede from the heart; *Phil. Trans.* for 1814, p. 595. . 0; Magendie says that the blood is warmed about 1° (cent.) in passing through the lungs; *Physiol.* t. ii. p. 397; and the same estimate of the different temperatures of arterial and venous blood is made by Thenard; *Chim.* t. iii. p. 671. Mr. Coleman, on the other hand, states the temperature of the two sides of the heart externally to be the same, but that the blood in the right ventricle is 1° or 2° warmer than in the left; he informs us, however, that the blood in the left ventricle cooled more slowly, thus indicating that it contained more latent heat; *On Suspended Resp.* p. 31, 2; 35, 6.

⁷ The results of Dr. Davy are not uniform, and although we have every reason to place full confidence in his statements, it

These experiments are, however, to be considered principally as relating to the theory of Crawford, so far as it depends upon the doctrine of the different capacities of arterial and venous blood. The opinion that animal heat results from the union of oxygen and carbon, as originally advanced by Black, and as subsequently illustrated by Lavoisier, although it may be obnoxious to the objection which has been already stated, is unaffected by the attempts that have been made to disprove the results of Crawford's experiments. We should still conceive that the heat must be produced by the formation of carbonic acid, although we might be unable to explain the exact mode in which it is set at liberty, or the precise point in the circulation where it assumes the form of sensible heat. We accordingly find that the chemical theory, or that which supposes respiration to be a

must be allowed that the experiments require to be more frequently repeated and varied, before we can draw any decisive conclusion from them; they, however, in three cases out of four, indicated a greater capacity in arterial than in venous blood; in those experiments in which he himself placed the most confidence, the relative numbers are 913 and 903; *Phil. Trans. for 1814*, p. 591..5. The inference that may be drawn from Mr. Coleman's experiments is in favour of the greater capacity of arterial blood; *On Suspended Respir. ut supra*. M. Magendie, in making a comparison between the properties of arterial and venous blood, has referred to the experiments of Dr. Davy on their respective capacities; *Physiol. t. ii. p. 288*; but it is remarkable that he has quoted that only in which the capacity of the venous was greater than that of the arterial, and which Dr. Davy considers as less to be depended upon than the other three; *Phil Trans. for 1814*, p. 595.

process more or less analogous to combustion, generally prevailed, until the doctrine was attacked in its essential point by a series of experiments that were performed by Mr. Brodie.⁸

Mr. Brodie took advantage of the process which was described in the last chapter for restoring the action of the heart, after decapitation or the removal of the brain, by artificially inflating the lungs. By this means we are able to produce the usual change in the appearance of the blood from the venous to the arterial state, when we find that oxygen is consumed and carbonic acid disengaged, as in natural respiration. The chemical action of the lungs being thus maintained, as in the state of health, it became a curious subject of inquiry, whether heat was also evolved, and the temperature of the animal supported, in the same manner as during the perfect state of the functions. Upon making the experiment this appeared not to be the case, for it was found that when, by any means, the influence of the brain was effectually cut off from the heart and blood vessels, although by means of artificial respiration the con-

⁸ There are various remarks in Hunter's *Essay on Animal Heat*, which appear inconsistent with the chemical hypothesis. See *Observ. on the Anim. Econ.* p. 103, et alibi; but those who are conversant with the writings of this physiologist must be aware, that we are not to deduce his doctrines from particular expressions, but from the general scope and tenor of his arguments. He, however, very explicitly states that the power of producing animal heat does not "depend upon the nervous system; for it is found in animals that have no brain or nerves;" p. 103, 4.

traction of the heart and the passage of the blood through the lungs is maintained, so that it apparently undergoes its change from the venous to the arterial state, yet the generation of animal heat seems to be suspended, and consequently, if the air which is inspired be colder than the body, the effect of respiration is to cool the animal.⁹ Mr. Brodie's experiments were so simple and appeared so decisive in their results, that the conclusion from them was thought to be irresistible, and notwithstanding the mass of evidence that had been accumulated in favour of the chemical theory of respiration, and the numerous arguments and analogies that had been adduced in its favour, the opinion of many of the most intelligent physiologists was, that respiration, as far as respects the production of carbonic acid, is not the cause of animal heat, and is only remotely connected with it, that the lungs have no immediate or direct concern in the operation, but that it depends upon the nervous influence.¹

⁹ Phil. Trans. for 1811, p. 37. 48; also Phil. Trans. for 1812, p. 378. Mr. Brodie found in his second set of experiments, that when the sensibility of an animal is destroyed by a narcotic poison, and the lungs artificially inflated, the power of generating heat is destroyed as effectually as by decapitation, while the power is restored in exact proportion to the return of the sensibility, when the influence of the poison ceased to act upon the system.

¹ Mr. Brande unequivocally gives it as his opinion, that the researches of Mr. Brodie have produced the "complete subversion" of the chemical doctrine of animal heat; Manual, v. iii. p. 226, Dr. Young says, that "animal heat depends jointly on

But notwithstanding the value of Mr. Brodie's experiments, there are two circumstances which seem to have been overlooked or disregarded by him, and which it will be necessary to examine, before we can draw the conclusion which, at first view, appeared so naturally to flow from them. In the first place we must inquire into the degree of cooling effect which is caused by the inflation of the cold air into the lungs, and compare this with the opposite effect which might be produced by the generation of the carbonic acid. In the natural or entire state of the animal, the respiration is regulated partly by the influence of volition, and partly of instinct, the quantity of air inspired being just sufficient to an-

circulation and nervous energy, but probably little on respiration," and in the list of authors that he subjoins, he designates Mr. Brodie's paper in the *Phil. Trans.* as the most important treatise on the subject; *Med. Lit.* p. 108. His opinion, however, upon this point seems to be somewhat vacillating, as in a subsequent passage, p. 503, he supposes that the chief use of the red particles of the blood, and "of the process which is carried on in the lungs, is to preserve the temperature of the body," and remarks, that the "process may be explained according to the ingenious and important theory of Dr. Crawford;" but adds, "that the nervous system appears to have an influence over the process, without which it cannot be carried on," p. 504, 5. Dr. Thomson also conceives that Mr. Brodie's experiments have entirely destroyed the foundation of Dr. Crawford's theory; *Chem.* v. iv. p. 632. Mr. Earle, who has brought forwards some very interesting pathological facts in support of the connexion between the nervous action and the evolution of animal heat, seems to consider the chemical theory as overthrown by Mr. Brodie's experiments; *Med. Chir. Tr.* v. vii. p. 173, et seq.

swer the demands of the system; but in the experiment which has been related, the air is forcibly sent into the pulmonary vesicles, without any co-operation from the animal, and, of course, independently of the correspondence between the different functions which are connected with the action of the lungs.² Now, as we have already observed, respiration may be considered as a compound process, one essential effect of which is to abstract heat from the body; the question therefore to examine is, whether the heating or cooling effect of respiration will preponderate, under the circumstances in which the animal was placed in these experiments. The second point in which Mr. Brodie's conclusion is premature, respects the part of the circulation in which the heat is extricated. If according to the theory of Crawford, it is not in the lungs, but in the capillary vessels, that this operation is carried on, we should scarcely expect to find the temperature of the animal supported by the artificial inflation, because the immediate effect in this case is to render heat latent, which is necessarily a cooling process, while it is by no means surprising that the disengagement of the latent heat, which takes place in the natural situation of the animal, should have been entirely prevented, or very much diminished, where the functions of the system generally, and those of secretion and assimilation in particular, were so much deranged, or even entirely suspended. Perhaps, therefore, it

² See the judicious observations of Dr. Philip; *Inquiry*, p. 205.

may not be going too far to assert, that in the case in question, where a quantity of cold air is forcibly propelled into the lungs, and a portion of heat necessarily rendered latent, by the conversion of venous into arterial blood, and where we may also presume that the air which is discharged from the lungs, would carry off a quantity of aqueous vapour, and in this way produce a farther abstraction of heat, while, on the other hand, it is not improbable, that the various processes by which the blood is venalized in the natural state of the system, must be impeded or deranged, the result which we ought to expect would be the cooling of the blood, as Mr. Brodie found it to be in his experiments.

These theoretical considerations might have induced us to pause before we admitted the conclusion, that the chemical change of the air in the lungs is not the source of animal heat; but we have two sets of experiments, one by Dr. Philip, and a second by Legallois, which directly oppose those of Mr. Brodie, by showing, that if the attending circumstances are duly considered, we can clearly trace a connexion between the diminution in the quantity of oxygen consumed and the deficiency in the evolution of heat, when we employ the process of artificial inflation. Dr. Philip conceived that the cause why the temperature of the body diminished more rapidly where this operation was employed, than where the animal was left undisturbed, depended upon too large a quantity of air having been propelled into the lungs, and he accordingly found that when a less quantity

of air was used, the cooling process was sensibly retarded by the inflation, so as directly to obviate Mr. Brodie's objections, and to exemplify the power of the lungs in generating heat.³ That this power is not directly dependent upon the nervous system appears also to be proved, by an experiment in which the rate of cooling in a dead animal, where the brain and nervous system had been removed, was compared with one which was left in its entire state, in which case, and also when the artificial respiration was employed, no difference could be observed in the temperature of the animals.⁴

A very elaborate and ingenious train of experiments was performed upon the same subject by Legallois, the results of which were decidedly favourable to the chemical theory, and fundamentally coincided with those of Dr. Philip. The experiments

³ Inquiry, c. 10. p. 197, et seq. The precise subjects of inquiry which Dr. Philip proposed in these experiments were; 1. Whether the *nervous power* is capable of evolving heat from the blood, after the *sensorial power* has been destroyed; and 2. Whether under these circumstances, more heat is evolved by artificially supporting the circulation, than by leaving the animal undisturbed. The animals employed in the experiments were rabbits, and they were killed by a blow on the occiput; a comparison was made both between the effect of *inflating the lungs with more or less rapidity*, and *inflating the lungs and leaving the animal undisturbed*, see ex. 64, 5, 6. We are informed that in one experiment the temperature was actually raised by nearly 1°; p. 199, 0. Dr. Hastings performed similar experiments with the same results, p. 200. See also Quart. Journ. v. xiv. p. 96, 7.

⁴ Inquiry, p. 101, 2. Ex. 67, 8, 9.

consisted in observing the rate of cooling in animals under different circumstances, and in comparing the effect of the inflation of the lungs upon perfect and upon mutilated animals, and also in noticing the degree in which animal temperature is influenced by various impediments to the respiration or to the action of the other functions.⁵ He deduced the following important conclusions from his experiments, 1. That the animals upon which artificial respiration has been employed, although they suffered a reduction of temperature, yet it was less by from 1° to 3° (cent.) than in simply dead animals; 2. That in cooling through a certain number of degrees, they parted with more heat than simply dead animals; 3. That inflation of the lungs of perfect and healthy animals lowers their temperature, and that if the operation be continued for a sufficient length of time, they may even be destroyed by cold; and 4. That the same effect may be produced by any circumstance which constrains or impedes the respiration. One important point still remained to be decided; when the cooling process is going forwards by any constraint or impediment to the respiration, is the consumption of oxygen proportionably diminished? Many causes conspire to render the investigation one

⁵ *Ann. de Chim. et Phys.* t. iv. p. 5, 113. In Legallois' work "*Sur la Vie*," he gives an account of the effect of artificial respiration, which he found to have the power of reducing the temperature, according to the observation of Mr. Brodie, although, as he conceived, not in so great a degree; *Avant-propos*, p. xx; also note in p. 241, 2.

of considerable intricacy, but by a series of well conceived experiments, which were varied and modified in such a manner as to meet the difficulties which successively presented themselves, the general conclusion seems to be well established, that the evolution of heat always bears a relation to the consumption of oxygen, and that the variations are not greater than might be naturally expected, from the complicated nature of the operations which are always going forwards in the animal œconomy.

As Legallois' experiments are many of them novel and ingenious, and lead to many curious results, it may be proper to give a brief account of some of the most important of them. After establishing the four positions that are mentioned above, he informs us that laying an animal on its back lowers its temperature, and he examined whether in this case the consumption of oxygen was diminished. The experiment was performed on rabbits and cats. For the purpose of comparison he first operated upon the animal while it was at liberty, and afterwards when confined on its back; the results are not quite uniform, but, for the most part, there was considerably more oxygen consumed when the animals were at liberty. Upon repeating the experiment, with certain variations, it appeared that when the respiration was in any way constrained, by the animal being tied on its back, or by there being a deficiency of air, less oxygen is consumed. It was found, however, that in certain cases, the cooling was more rapid than ordinary, even when more oxygen is consumed, owing, as the author con-

jectures, to the struggles which are made, carrying off a portion of the heat. In order, therefore, to render the experiment still more decisive, he rendered the respiration so difficult, that no voluntary effort of the animal could enable it to consume the usual quantity of oxygen; this was accomplished either by placing it in air that was much rarefied, or by mixing with it a large proportion of nitrogen.⁶ The experiment was then performed in this way upon dogs, cats, rabbits, and guinea-pigs, when the greatest cooling effect was always found to correspond with the least consumption of oxygen. We are informed that the quantity of carbonic acid formed is always less than that of the oxygen which disappears. When a quantity of carbonic acid is mixed with the air, this is in part removed, so as to prove that carbonic acid is absorbed by the lungs. The author's final decision is, that the nervous system affects the temperature, but that it is by an indirect operation, so far as it contributes to bring the air into contact with the blood. We may therefore conclude upon the whole, that although there are many difficulties which attach to particular parts of the subject, the source of animal heat is in the action of the air upon the blood, and that it ultimately depends upon the abstraction of carbon from this fluid, and the conversion of oxygen into carbonic acid.

As to the question, why the union of oxygen and carbon produces heat, I have already remarked, that

⁶ *Ann. Chim. et Phys.* t. iv. p. 21. et seq.

it is one which belongs rather to chemistry than to physiology; it is sufficient for our purpose to state, that no greater difficulty occurs in one case than in the other. With respect to the second position of the theory, the mode in which the heat is distributed through the various parts of the body, it is admitted, that we are still unable to form a decisive conclusion. After attentively perusing the experiments of Crawford, and comparing them with those that have been performed with a contrary result, I confess that the balance of evidence appears to me to be greatly in favour of the former, but I acknowledge, that they are of so delicate a nature, as not to be entitled to implicit confidence, and that it would be extremely desirable to have them carefully repeated.

If we consider the subject upon general grounds, we shall find that there are many circumstances which afford a strong presumption in favour of the chemical doctrine, by tending to establish an intimate connexion between the functions of respiration and calorification. In the first place, all animals that have a temperature, much superior to that of the medium in which they are immersed, have their lungs constructed in the most elaborate manner. What are styled the warm-blooded animals, have the organs of a large size, and so formed as to permit the air and the blood to come into the most extensive proximity, and thus to exercise the most powerful influence upon each other. In the amphibia, the lungs are furnished with fewer vessels and with larger air-cells, at the same time that only a part of

the blood passes through them at each circulation, and their temperature is consequently found to be proportionally low. In fishes, the quantity of blood which passes through the gills to receive the action of the air, and the chemical effect produced upon it, is still smaller, and it is found that the temperature of this class of animals differs but a degree or two from that of the water in which they are immersed. It appears, in short, that if we compare the different classes of animals with each other, we, in all cases, perceive a strict relation between their temperature, and the quantity of oxygen which they consume. It is farther observed, that in the same class of animals, or even in the same individual, whatever quickens or impedes the passage of the blood through the lungs, or whatever promotes or retards the action of the air upon the blood, in the same proportion increases or diminishes the temperature of the animal, so to afford a strong argument in favour of the opinion, that these operations bear to each other the relation of cause and effect.

I shall enumerate a few circumstances, principally taken from Dr. Edwards, which, although of rather a miscellaneous nature, may be properly classed together, as they all bear indirectly upon the question of the connexion between animal temperature and the consumption of oxygen by the lungs. The phenomena of hybernation, as they were related in the last chapter, p. 197, confirm this connexion, as we find in all cases, that exactly in proportion to the diminution of the chemical effect upon the air, so is the decrease

of animal heat ; a fact which is abundantly proved by the references that were made to Hunter, Spallanzani, Carlisle, and others. These authors all agree, that the temperature of the external parts of the body is nearly reduced to that of the surrounding medium, while the internal parts, as well as the blood and the vital organs, are not more than 1° or 2° higher. But we have an experiment of De Saissy's, related by Dr. Edwards, which is peculiarly illustrative of this point, for he found that he was unable to bring an hybernating animal into the torpid state, by the reduction of temperature alone, without also constraining the respiration.⁷ Dr. Edwards informs us, that such of the mammalia as are born with the ductus arteriosus large and open, have less power of producing heat, but that in proportion as the duct closes, their power of generating heat is increased ; and that when individuals of the same species have the ducts more closed than usual, their temperature is more stationary.⁸ He found by experiment, that after making due allowance for their bulk, young animals consume less oxygen than adults, and that they have a less power of generating heat, the production of heat and the consumption of oxygen being in all cases proportionate.⁹ I have already adverted to the observation of Buffon, that a newly-born animal can live for a certain length of time under water ; the fact was confirmed by Dr. Edwards, but he found that this power belonged only to those animals which, while

⁷ De l'Influence, &c. p. 152. ⁸ Ibid. p. 618. ⁹ Ibid. p. 190. . 3.

young, generate the least heat, that they soon lose this power, and at the same time acquire the capacity of evolving heat in a greater degree; we may presume that this difference depends upon the state of the ductus arteriosus, and the consequent state of the pulmonic circulation. We learn also that young animals differ from each other in their power of resisting cold, or, which amounts to the same thing, of generating heat, and in these cases we can always perceive a relation between this power and the consumption of oxygen.¹ In addition to these considerations I may adduce the experiments of Provençal on the effects of dividing the par vagum, of which I have already given an account, p. 168, where it was found that the temperature declines in exact proportion to the difficulty which the air has in getting access to the pulmonary vesicles. The curious observation of MM. Prevost and Dumas, according to which the temperature of the different classes of animals is proportionate to the number of the red particles in their blood, also affords a farther confirmation of the same doctrine; for we can conceive of no other kind of action that these particles can exercise, except a chemical one upon the air.² It had been frequently remarked that there was a certain degree of correspondence between the temperature of an animal and the colour of the blood, but the observation had been previously made in a general way only. The lower temperature of the "Cœruleans," see p. 117, is

¹ De l'Influence, par. 4. c. 2.

² Ann. Chim. et Phys. t. xxiii. p. 64. et seq.

likewise an argument in favour of the chemical doctrine of animal heat.³ Upon the same principle we account for the lower temperature of asthmatic patients. Dr. Bree informs us that they are generally colder than other individuals; he particularly mentions one case where the temperature during the paroxysm sunk to 82° , while at other times it was 97° ; and he believes that there are instances in which it is lower.⁴ When from any cause the conversion of arterial blood into the venous state, is impeded, as was observed by Hunter to take place during fainting, the temperature is also lowered.⁵

It may be farther urged as a strong argument in favour of the chemical doctrine of animal heat, that in consequence of the union of oxygen and carbon in the lungs, heat must necessarily be extricated, which can be no otherwise disposed of than in raising the temperature of the animal, although it is admitted that there is a difficulty in explaining in what exact part of the system the extrication of heat takes place.⁶ Nor are we obliged to rest upon the general fact only: it is proved by decisive experiments, that under ordinary circumstances, the formation of the carbonic acid, which is produced by respiration, is an actual source of heat, which may be exactly measured, and

³ Blumenbach, *Inst. Physiol.* note to § 167.

⁴ *On Disordered Respiration*, p. 137, 8.

⁵ *On the Blood*, p. 68.

⁶ Mr. Brodie's experiments, as he correctly observes, clearly prove that the nervous influence is not necessary to the production of the chemical changes which the air and the blood naturally produce upon each other in the lungs; *Phil. Trans.* for 1812, p. 389.

which must be available in maintaining the temperature of the animal at the required standard. By means of the calorimeter, Lavoisier and La Place compared the heat evolved during the formation of a given quantity of carbonic acid, as produced by the combustion of carbon, with the heat evolved during the formation of the same quantity of carbonic acid by respiration, and the amount of absolute heat appeared to them to be at least as great in the latter case as in the former.⁷

A series of experiments has been lately performed by M. Dulong, on the question how far the heat extricated by the generation of the carbonic acid in the lungs is adequate to the supply of the caloric which is expended by the body under ordinary circumstances. From the mode in which the experiments were performed, as well as from the acknowledged talents of the author, we cannot but consider

⁷ Mem. Acad. Scien. pour 1780, p. 407. et seq.; see also Menzies on Respiration, p. 39. et seq.; Thenard, *Traité*, t. iii. p. 670, 1. De la Rive, *De Calore Animali*, gives the following estimates; the heat employed in evaporating the water that is discharged from the lungs is sufficient to melt 3.5 lbs. of ice, that necessary to raise the temperature of the inspired air 29 lbs. making together 32.5 lbs. The heat which is evolved by the formation of the carbonic acid would melt 108 lbs. of ice, that by the absorption of oxygen in the lungs, besides what is employed in forming the carbonic acid, would melt 26 lbs., making together 134 lbs.; hence we have the heat which would be required to melt 101.5 lbs. of ice left to maintain the temperature of the body. The three first of the above quantities are pretty correctly ascertained; with respect to the fourth, it would be almost impossible to form an average of its amount.

the results as entitled to much attention. The apparatus appears to have been skilfully contrived, so as to allow the animal to remain as little under restraint as possible, while it was capable of measuring with accuracy the quantity of heat that was evolved by respiration; and in order to render the results less liable to objection, six different kinds of animals were employed.

The conclusion that M. Dulong draws from his experiments, is that the quantity of caloric disengaged by the lungs, although considerable, and an actual source of temperature, is not sufficient to account for the whole of the animal heat.⁸ But we may remark, that with all the care which was bestowed upon the experiments, and valuable as they must be considered, there are still some points that require farther inquiry. In the first place, as the author is himself aware, his conclusion essentially depends upon the correctness of the estimate, which was made by Lavoisier and Seguin, of the quantity of heat extricated during the combustion of a given weight of charcoal. For this purpose these philosophers employed their calorimeter, an instrument, which, although highly ingenious, we have reason to suppose, is not capable of affording very correct results.

In the second place, it appears that, besides the oxygen which was expended in the formation of the carbonic acid, M. Dulong found that a considerable

⁸ Magendie, *Journ. de Physiol.* Jan. 1823.

quantity, in some cases amounting to one-third of the whole, was expended in some other way; but in whatever manner it was employed, in losing its aeriform state, it must evolve a quantity of caloric, which should be taken into account in forming our estimate. It may be farther observed, that we cannot be permitted to assume that the same quantity of heat will be extricated by the combustion of charcoal heated in oxygen gas, as by the union of the carbon emitted by the lungs with the oxygen of the inspired air; and, lastly, it is not unfair to suppose that, notwithstanding all the care bestowed upon the experiments, there might be various circumstances connected with the actions and organization of the animal, which would affect the regularity of the functions and their relation to each other, and which might, in various ways, interfere with the results.

But even were we to admit the conclusion of M. Dulong, without any restriction, it will appear that we have still a large quantity of heat extricated in the lungs, which must necessarily raise the temperature of the body, although there may be some reason to doubt, whether it be adequate to the whole of the effect which is produced. Upon the whole, however, I conceive, that the information which we possess on the subject will lead us to the opinion, that, under the ordinary circumstances in which the animal is placed, the heat extricated by the lungs is sufficient to maintain its temperature, the difficulty will therefore be not to find a source for the heat which animals possess under ordinary circumstances, but to

account for the deviations which occur in extraordinary cases, or in unnatural situations of the individual. These deviations, it is admitted, we frequently find it very difficult to explain, but they cannot be allowed to form any objection to the general doctrine, which appears to be founded upon a fair induction from a sufficient number of direct and well established facts.

To the deviations which are occasioned by the general state of the system, independent of mere chemical action, may be referred some interesting observations of Dr. Edwards, in the comparative power which young animals possess of producing heat. In opposition to the general opinion, that their temperature is superior to that of the adult, he found that young animals indicate a lower degree of heat;⁹ and that in some cases, as that of the puppy, kitten, and rabbit, if they be removed from the mother, and exposed to an atmosphere of between 50° and 70°, their temperature will sink to nearly the same degree. This imperfect power of producing heat exists in the greatest degree immediately after birth, and progressively diminishes, until, in about 15 days, they acquire the faculty of generating heat in the same degree with the adult.¹ The observation does not,

⁹ Vide supra, p. 246.

¹ There is a difference of opinion respecting the temperature of the human infant, which must be removed by farther observations. Haller remarks, in conformity with the common opinion, that children have less power of producing heat than adults; *El. Phys.* vi. 3. 10; and this is confirmed by the obser-

however, apply to all the mammalia; this defect appeared to be confined to those animals that are born blind, thus indicating a state of imperfection in the general state of their organs and functions. The case is the same with birds as with the mammalia; i. e. some birds which are born in a defective state, with respect to their organs generally, have a defective power of producing heat, whereas those that are born in a more perfect condition, have the respiratory organs also more capable of exercising their functions. Dr. Edwards correctly points out the analogy which these animals bear to the hybernating tribes; they may both of them be considered as the connecting links between the two great divisions of the warm and the cold-blooded.²

In the last place it may be urged, that if we reject the chemical doctrine of animal heat, we have no adequate hypothesis to substitute in its place; for although it is admitted that, in some way, which we are not very well able to explain, the nervous system exercises a powerful influence over the extrication of heat, we have no analogy which would induce us to suppose that the nerves can directly produce a chemical change, or that they can alter the chemical relation of substances to each other, unless by the

observations of Dr. Edwards and Despretz, see above, p. 246. We have, however, on the contrary, the direct testimony of Dr. Davy, that the temperature of young animals generally, and that of a newly-born child, which he particularly examined, is higher than in the adult; *Phil. Trans. for 1814. p. 602, 3.*

² De l'Influence, &c. par. 2. c. 1.

intervention and introduction of some new chemical or physical agency, of which, in this case, we have no intimation, and of the nature of which we can form no plausible conjecture.

An exception to the above remark may appear to be deducible from the curious discovery of Dr. Philip, respecting the action of galvanism upon the blood. We learn from him, that if the galvanic influence be applied to fresh drawn arterial blood, an evolution of heat, amounting to 3° or 4° , takes place, while the blood assumes the venous hue, and becomes partially coagulated; it appears also that this extrication of heat cannot be produced from venous blood, nor even from arterial blood, except immediately upon its being discharged from the vessels.³ Dr. Philip draws an inference from the experiment in favour of his hypothesis of the identity of the nervous and the galvanic influence, for, as he supposes that one of the functions of the nervous system is to assist in maintaining the temperature of the body,⁴ it appears that he have here, in like manner, the evolution of heat produced by the application of galvanism.

I shall reserve my observations upon this hypothesis to the next chapter, which treats upon secretion, but it will be necessary to make a few remarks in this place upon the experiment in question, so far as it relates to the subject of animal temperature. Although there is considerable difficulty in explaining

³ Inquiry, p. 168, et seq.; Quart. Jour. v. xiv. p. 105. et seq.

⁴ Inquiry, ex. 76. . 82. p. 238. et seq.; Quart. Jour. v. xiv. p. 106.

the intimate nature of the galvanic action, yet we know that its specific effect is to induce certain chemical changes in the substances to which it is applied, and that so far as these changes are concerned, it is to be regarded in no other light than as a chemical agent. As the evolution of heat is, strictly speaking, a chemical effect, we must inquire what difference there is between arterial and venous blood, and between arterial blood when fresh drawn, and the same fluid after it has been for some time discharged from the vessels, which might be supposed to be affected by the chemical action of the galvanic apparatus. With respect to the first of these points, we may go so far as to say, that the various experiments which have been performed on the subject lead us to suppose, that the arterial blood contains an excess of oxygen and the venous of carbon. Dr. Philip's experiments therefore show us, that the galvanic influence has the power of acting upon, or decomposing, oxygenated blood in a greater degree than carbonated blood, but, except this simple fact, it may be doubted whether any further influence can be deduced from them. There is much more obscurity in the change which the blood undergoes when it leaves the vessels, but it is certain that its chemical and physical properties begin almost immediately to experience an alteration; and it seems that this alteration, whatever it be, renders it less liable to be decomposed by the action of galvanism. Upon the whole, however, I may observe that Dr. Philip's experiments, so far as we are able to comprehend their nature, are favourable

to the chemical doctrine of animal heat. They prove to us that heat is evolved from blood by a chemical operation, also that it is more easily evolved from arterial than from venous blood, or rather that when the blood becomes venalized, it is no longer capable of giving out heat by the same process which evolves it from arterial blood.

I shall give a brief abstract of the opinions of some of the most eminent physiologists respecting the chemical doctrine of animal heat, in addition to those that have been already noticed. Menzies gives his assent to the hypothesis, and offers various arguments in support of it;⁵ and we have the same in Fourcroy.⁶ Mr. Ellis offers various considerations in favour of the doctrine, principally derived from the correspondence between the temperature of the animal and the extent of the lungs, or the facility with which the blood is transmitted through them.⁷ Dr. Henry gives his full assent to Crawford's theory;⁸ and Mr. Dalton, after discussing the nature of the objections that have been urged against it, concludes that it is not affected by them.⁹ M. Thenard ascribes animal heat to the union of the oxygen of the air with the carbon of venous blood;¹ M. Magendie supposes that the combination of the oxygen of the air with the carbon of the blood is sufficient to explain most of the phenomena of animal heat, although he conceives there are certain facts which cannot be accounted for upon this

⁵ Essay on Resp. p. 36. et seq.

⁶ System, v. x. p. 524, 5.

⁷ Inquiry, p. 233, 240. et alibi.

⁸ Elements, v. iv. p. 407.

⁹ Manch. Mem. v. ii. (2d ser.) p. 38, 9.

¹ Chim. t. iii. p. 670.

principle. The only circumstance to which he refers is the increased temperature of the blood drawn from a part suffering under local inflammation, which, as he says, is considerably above that of the blood in the left side of the heart; but the fact is stated generally without any reference.² Dumas remarks upon the correspondences between the capacity and structure of the lungs and the temperature of the animal, and hence draws an inference in favour of the chemical theory. He, however, considers it an objection to this doctrine, that the temperature of the animal remains the same, whatever be that of the surrounding medium; he also thinks that the quantity of oxygen consumed in respiration is not sufficient to serve for the support of the temperature, and for the removal of the vapour and carbonic acid; but his objections are brought forward in a general manner, without reference to any particular facts.³ Legallois, at the conclusion of the experiments, of which an abstract is given above, p. 271, although he supposes that the nerves have an indirect effect upon the evolution of animal heat, states his opinion to be, that when the blood is arterialized its capacity is increased, and that when it is venalized in the capillaries its capacity is changed, and its heat given out; and adds that when, from any cause, the respiration is rendered laborious, by the proper quantity of oxygen not being admitted to the lungs, the temperature is proportionally reduced.⁴

² *Physiol.* t. ii. p. 400.

³ *Physiol.* t. iii. p. 115, 121., 171.

⁴ *Ann. de Chim. et Phys.* t. iv. p. 118. et seq.

Seguin brings forward various considerations in favour of the connexion between animal heat and the chemical change which the blood experiences in respiration;⁶ and Sœmmering supports the chemical doctrine of animal heat.⁷ Cuvier supposes that respiration causes the combination of oxygen with carbon and hydrogen, in consequence of which heat is disengaged. He says, "*Le poumon est le foyer de la chaleur animale, et c'est là que le sang puise celle qu'il porte dans le reste du corps.*"⁸ This chemical doctrine of animal heat, in its essential parts, is also adopted by Blumenbach, although he introduces some modifications into Crawford's theory; he supposes that the blood leaves the lungs charged with latent heat, in consequence of the oxygen which it contains; that it acquires carbon in the small vessels, and sets its latent heat at liberty;⁹ and Dr. Elliotson, likewise, although aware of the force of the objections that have been urged against it by Mr. Brodie and others, admits that "many circumstances favour it;" and appears, upon the whole, disposed to adopt it.¹ I may also remark concerning Dr. Philip's opinion, that although he regards the nervous system as a necessary instrument in the process of calorification, yet we may infer, that he supposes the immediate effect to be produced by the union of oxygen and carbon.² Sir A. Carlisle remarks upon the correspondence

⁶ Ann. Chim. t. v. p. 259, O.

⁷ Corp. Hum. Fab. t. vi. § 76. in the chap. on Respiration.

⁸ Tab. Elem. p. 46.

⁹ Inst. Phys. § 167. p. 97, 8.

¹ Inst. Phys. p. 101. 4.

² Inq. p. 250. et alibi.

between the comparative anatomy of the lungs and the temperature of the animal.³ Dr. Davy's general conclusion is against Crawford's hypothesis, because he finds no material difference between the capacities of arterial and venous blood, that the temperature of the left side of the heart is higher than that of the right, and that the temperature of the parts of the body generally diminishes as we recede from the heart. These facts, he observes, are more agreeable to Black's hypothesis, although they may be explained on Mr. Brodie's; but upon the whole, he inclines to Black's.⁴

§ 2. *Of the means by which the Animal Temperature is regulated.*

Having endeavoured to establish the doctrine that the source of animal heat is in the lungs, and that it depends upon the chemical action of the air on the blood, the next point for our consideration will be the mode by which the uniformity of the animal temperature is preserved. We find that, among the warm-blooded animals, to whatever degree of heat or of cold the body is exposed, the blood and the internal parts always indicate nearly the same degree of heat.⁵ According to the view which has been

³ Phil. Trans. for 1805. p. 15.

⁴ Phil. Trans. for 1814. p. 600. .3.

⁵ The latest observations, and we may presume the most correct, that we possess on this subject, lead us to conclude, that this uniformity is not quite so great as was formerly supposed, especially when we arrive at a temperature nearly equal,

taken above of the cause of animal heat, the question in its strict form will be, by what means is the combination of oxygen and carbon in the lungs so regulated, as that the evolution of heat is in proportion to the demand for it in the system.

But in order to solve this problem, a previous inquiry presents itself; is the combination of oxygen and carbon in the lungs always going forwards at the

or superior to, that of the body itself. This is decidedly proved by the experiments of Delaroche and Berger, who found that by exposing warm-blooded animals to very high temperatures, they experienced an elevation equal to 7° or 8° (cent.) Journ. Phys. t. lxxi. p. 289. et seq. Dr. Edwards's observations on birds at the different seasons of the year, showed that there was a difference of 4° (cent.) between the winter and summer months; De l'Influence, &c. p. 489. Dr. Davy observed that the temperature of the inhabitants of Ceylon, was 1° or 2° higher than the ordinary standard; *ibid.* The uniformity of the animal temperature appears to be more steadily maintained in great degrees of cold, where it seems that the heat of the internal parts is scarcely diminished, as long as the functions proceed in their ordinary course. We have many accounts to this effect by travellers and naturalists, but none, perhaps, upon which greater reliance can be placed, than upon that of Capt. Lyon, contained in Capt. Parry's Second Voyage to the Arctic Regions, p. 157. The observations were principally made upon newly killed foxes, the temperature of which was found to be from $106\frac{3}{4}^{\circ}$ to 98° , that of the air being from 3° to 32° ; it did not appear that there was any relation between the temperature of the air and of the animals. The observations were made at Winter Isle, N. lat. $66^{\circ} 11'$. So far as regards the sensations, the experience of these voyagers proved that the state of motion or rest in the atmosphere had a very great effect in exciting the feeling of cold; they found a calm air of -50° more tolerable than a breeze at 0° .

same rate, and consequently adapted to the lowest temperature, which is consistent with the continuance of life, or is it found to vary in the inverse proportion to the temperature of the animal? And there is a farther consideration which we must now enter upon; when an animal is placed in a medium, the temperature of which is higher than that natural to itself, we find that the body still remains at nearly its ordinary standard. We have therefore in this case, not merely the suspension of the process by which heat is generated, but it would appear that the contrary effect must be produced, that the body must possess the power of resisting heat, or of actually generating cold. This leads us to the third question which was proposed in the commencement of the chapter; by what means is the body cooled at high temperatures? And as there would appear to be an intimate connexion between this inquiry and the former, it will be more convenient to investigate them in conjunction with each other.

The cases in which the body is exposed to a temperature greater than what is natural to it, are so rare compared to the contrary occurrence, and the effects of such exposure are, in various ways, so unfavourable to the exercise of the vital functions, that it was formerly assumed as an acknowledged matter of fact, that life could not exist under such circumstances. Boerhaave conceived that he had proved this point by direct experiment; but from causes which we cannot now ascertain, there must have been some source of inaccuracy, which interfered with his

results, and led him to an erroneous conclusion.⁶ The first well authenticated facts which disproved this opinion, were communicated by Tillet and Duhamel, who gave an account of some young women, the servants of a baker at Rochefoucault, in Angoumois, who were in the habit of going into the heated ovens, in order to prepare them for the reception of the loaves.

The temperature to which they were exposed seems to have been, in some cases, as high as 278° Fahr., and this heat, it is said, they were able to endure for 12 minutes without any material inconvenience, provided they were careful not to touch the surface of the oven.⁷ The statement excited great astonishment, and at the time was scarcely credited, but it is fully confirmed by subsequent observations. A series of experiments were performed by Fordyce, Blagden, and others, where a chamber

⁶ The experiments were performed by Fahrenheit at the suggestion of Boerhaave, and the result is stated to have been that animals could not support a heat of more than 146°; Boerhaave, *Chem. t. i. p. 275, 6.* Various accounts were, from time to time, published by travellers, of the high temperature to which the body is subjected in tropical climates, which were often many degrees above that of the human body. See Blumenbach, *Physiol. p. 97*; Lawrence's *Lect. p. 206*; and Delaroche, in *Journ. Phys. t. lxiii. p. 207.* We have some curious observations by Dr. Edwards on the effects of different temperatures on the cold-blooded animals; a frog which can live for 8 hours immersed in water at 32°, is destroyed in a few seconds in water at 105°; this appears to be the highest temperature which cold-blooded animals are able to bear.

⁷ *Mem. Acad. Scien. pour 1764, p. 186. et seq.*

was heated to a temperature considerably above that of boiling water, in which these gentlemen found they could remain without much inconvenience for almost an indefinite length of time.⁸ Another set of experiments of the same nature were performed at Liverpool by Dobson with similar results.⁹ These experiments are valuable, inasmuch as they very fully establish the fact, and prove that there was no deception in the case, while they accurately mark the degree of heat both by the thermometer, and by its effect on inanimate substances that were exposed to it. But it is to be regretted, that the attention was almost exclusively confined to this point, and that while they were observing its action on the surrounding bodies, they almost entirely neglected to notice its effects on the living system. They indeed inform us that the temperature of the body was little,

⁸ Phil. Trans. for 1775, p. 111. et seq. The conclusion which Blagden draws is, that, "No attrition, no fermentation, or whatever else the mechanical and chemical physicians have devised, can explain a power capable of producing or destroying heat, just as the circumstances of the situation require;" p. 122. In a second paper in the same volume, p. 484. et seq., he observes, that although the evaporation from the surface might have some effect, it was "by no means sufficient to account for the whole of the cooling;" p. 488.

⁹ Phil. Trans. for 1775, p. 463. et seq. In one of the experiments related by Dobson, a young man remained in a temperature of 224° for 10 minutes; p. 464. In the second set of experiments performed in London, Blagden remained for several minutes in a temperature of 260°; this, however, it will be observed, is considerably less than the heat to which the young women in France were exposed.

if at all raised,¹ and that in some of their experiments a profuse perspiration took place; but even this is stated in a general way, so as not to give us any certain grounds for concluding whether it had any effect in lowering the temperature. This is the more remarkable, as Franklin had, some years before, with his usual penetration, suggested that the evaporation from the surface might be a means of diminishing the temperature of the body when exposed to great heats.² With respect to the effect of evaporation, the observations of Fordyce and his friends, imperfect as they were, seemed to be adverse to the hypothesis of Franklin; and the same conclusion was likewise formed by Crawford, who conceived that Fordyce had sufficiently proved, that evaporation could not account for the cooling effect which had been observed by him in his experiments. Crawford himself explained it upon the principle, which has been already referred to, that in proportion to the elevation of the temperature to which the body is exposed, the blood becomes less venalized, and, in the same proportion, loses the property of evolving heat, in consequence of its containing a less quantity of inflammable matter.³

¹ Phil. Trans. for 1775. p. 487..9; in this experiment, where the body was exposed, nearly naked, to a temperature of 220°, we are informed that a profuse perspiration took place.

² Letter to Lining, Works, v. ii. p. 85; Journ. Phys. t. ii. p. 454. et seq.

³ Phil. Trans. for 1781, p. 487..1; On Animal Heat, p. 382 t. 9. Some sensible observations were made upon the experiments of Fordyce, shortly after they were performed, by Chan-

Some ingenious observations on the effect of high temperatures were made by Bell of Manchester, in which, although he admitted the correctness of the facts, he endeavoured to prove that the operation of the heated air upon the body would be considerably less than might have been expected, because in proportion as the air is heated, it is so much expanded, that comparatively few particles will come into contact with the surface of the body;⁴ hence we find that while there was little difficulty in resisting the heat, when the body was in contact with the air alone, it was impossible to touch any solid substance without exciting pain, so that even the clothes soon acquired a temperature, which rendered it difficult to bear them without inconvenience. Bell's remarks, however, although they diminished the difficulty, did not remove it, and it was not until the subject was

geux, *Journ. Phys.* t. vii. p. 57. et seq., pointing out their imperfection in respect to the physiological inferences that might be deduced from them.

⁴ Manchester Memoirs, v. i. p. 1, et seq. He supposes that three circumstances may concur to prevent the operation of the high temperature; 1. The rarefaction of the air; 2. The evaporation; and lastly, The afflux of the colder fluids from the central parts of the system to the centre. We learn from Crawford, *Phil. Trans.* for 1781, p. 484, that Monro attributed the effect principally to this latter circumstance. Some curious facts that were noticed by Currie, respecting the effect of the application of water at different temperatures to the surface of the body, may be referred to the abstraction of heat by evaporation; to the same cause may, in a great measure, be ascribed the different cooling powers of fresh and salt water; *Phil. Trans.* for 1792, p. 199. et seq.

investigated by Dr. Delaroche, that we could be considered as possessing any real insight into this intricate subject. We are indebted to this ingenious physiologist for three valuable memoirs, which were published in succession,³ and which each of them considerably advanced our information on the effect of high temperatures upon the body, and on the means by which animals are cooled, when exposed to a degree of heat greater than their own.

His first paper contains an account of a series of experiments performed on himself and Dr. Berger, in which they were exposed to a temperature of between 225° and 228° , during which they noticed the amount of the evaporation, and also the chemical change produced upon the expired air. Experiments of similar nature were also performed on various animals, from which it appeared that the effects differed considerably in the different species, but that, for the most part, the larger suffered more than the smaller animals. It has been already observed that the experiments of Crawford, Jurine, and Lavoisier, seemed to show, that at high temperatures there is a less consumption of oxygen in the process of respiration, and this was generally regarded as, at least, one means by which the temperature of animals is regulated; but Dr. Delaroche informs us, on the

³ See for the 1st paper, *Journ. Phys.* t. lxiii. p. 207. and *Nicholson's Journ.* v. xvii. p. 142. 215; for the 2d paper, *Journ. Phys.* t. lxxi. p. 289. and *Nicholson's Journ.* v. xxxi. p. 361; for the 3d paper, *Journ. Phys.* t. lxxvii. p. 1. They were published respectively in the years 1806, 1809, and 1812.

contrary, that he was not able to observe any relation between the temperature of the air, and the degree of its deoxidation.

In his second paper, he more minutely attended to the temperature of different kinds of animals when exposed to great degrees of heat; he found it to be not so stationary as had been generally supposed, being frequently raised by 10° or 13° above the natural standard, although it still remains very much below the temperature in which an animal can live for a certain length of time without any considerable uneasiness. With respect to cold-blooded animals, although their heat is much less stationary than those with warm blood, he found that in high temperatures it fell considerably below that of the medium; frogs, for example, in air heated from 110° to 115° , were no more than 80° or 82° . He entered upon a second set of experiments for the purpose of directly ascertaining the effect of evaporation upon the temperature of animals, the results of which fully confirmed his former opinion, pointing out an obvious relation between the amount of evaporation and the degree in which the animals were able to resist great heats. One set of experiments consisted in enclosing various animals, for example, rabbits, guinea-pigs, and pigeons, in a box which was filled with steam; it was found that when the temperature of the apparatus was equal to, or greater than that of the body, the temperature of the animal was raised, and the inconvenience which it suffered was proportionally great.

In his third paper, Dr. Delaroche more particularly directs his attention to the chemical effects of respiration upon the air under different circumstances. Having proved in the former paper that when the evaporation from the skin and lungs is prevented, the cooling process is suspended, it was an interesting subject of inquiry whether, in this case, the chemical effects of respiration continue with as much activity as under ordinary circumstances. The experiments which were performed for the purpose of ascertaining this point, seem to have been sufficiently numerous and well conducted, and the result was, that the consumption of oxygen is greater as the temperature is lower, upon the average, about as 6 to 5. The formation of carbonic acid does not, however, follow the same ratio with the consumption of oxygen, being not only in all cases less, so as to indicate an absorption of oxygen, but it was found that this excess, or the difference between the oxygen consumed and the carbonic acid produced, was less at a high than at a low temperature, amounting upon the average to no more than one-tenth, or not more than half of the difference of the consumption of oxygen at different temperatures. The opinion of Crawford, Jurine, and Lavoisier, appears therefore, to a certain extent, to be thus confirmed, but Dr. Delaroche supposes that the effects observed in these experiments were not sufficient to explain the equalization of the temperature, without the aid of the cooling process of evaporation, and indeed this had been so fully established in the former experiments as to leave no doubt of its agency.

Our general conclusion will therefore be, that at high temperatures there is less caloric actually evolved, but that the circumstance which principally contributes to equalize the heat is the cutaneous and pulmonary evaporation, while at temperatures above that which is natural to the animal, the cooling process must be entirely ascribed to this operation.⁶

But, although we are very much indebted to Dr. Delaroche, there are still some points that require farther investigation. It is proved that evaporation abstracts heat from the body, but it still remains somewhat doubtful, whether the effect thus produced be adequate to the demands of the system, or whe-

⁶ Black very explicitly states his opinion that the heat which is necessarily absorbed in spontaneous evaporation, contributes to enable the body to bear the warmth of tropical climates. He particularly adverts to Fordyce's experiments, and states his opinion that it was owing to the evaporation from the surface that he was able to endure so high a temperature; *Lectures by Robinson*, v. i. p. 214. Lavoisier, in the course of his researches into the nature of respiration and animal heat, frequently refers to the mode by which it is equalized, when the body is exposed to different temperatures. He particularly notices this subject in his paper on transpiration, published in the *Mem. Acad.* for 1790; he defines this function to consist in "a loss of moisture, which requires heat to dissolve it in the air, and which by the cold thus produced, prevents the temperature from rising above the degree natural to the animal." This opinion was generally adopted by his contemporaries, but it could scarcely be regarded as more than a probable conjecture before the experiments of Delaroche. See Gregory's *Conspectus*, § 578; the *Observations on Animal Heat*, from § 571 to § 580, contain a succinct and elegant epitome of the doctrines which were the most approved when the work was published.

thier there may not be some other means employed which may co-operate to the same end. It would be desirable to examine with more minuteness than has hitherto been done into the quantity of vapour formed by the body as compared with the cooling effect produced. We should ascertain what temperature would be acquired by an inanimate mass of matter of the same bulk and capacity for heat with the animal body, and compare this with the actual temperature gained, and this again with the quantity of vapour generated. It would also be desirable to ascertain with more minuteness what is the exact effect of the respiratory organs of animals at a temperature higher than what is natural to them, first when the process of evaporation is suffered to proceed, and afterwards when it is suspended; is there any oxygen consumed under these circumstances, or to what amount, or in what degree, does the quantity differ in the two cases? The supposed power of the animal system in producing cold is one that has been treated of in a singularly mysterious manner, and much vague and indeterminate speculation has been employed upon a subject with which the framers of the hypothesis appear to have been very inadequately acquainted. The information which we derive from the experiments of Dr. Delaroche, although not in every respect complete, is, however, of great value; and it enables us to trace out a connexion between the different functions of the lungs, so as to afford, perhaps, the most interesting example which occurs in the animal œconomy, of that beautiful adjustment of the functions

to each other, upon which I have had occasion to remark in other parts of my work; for it appears that not only have the lungs the power of evolving heat in greater or less quantity in proportion to the demands of the system, but that the same organs, under other circumstances, can produce the directly contrary effects, and actually generate cold.

The researches of Dr. Edwards afford us some valuable information on the property which the system possesses of equalizing its temperature under different circumstances. He found that warm-blooded animals have less power of producing heat, after they have been for some time exposed to an elevated temperature, as is the case in summer, while the opposite effect is produced in winter. A series of comparative experiments were performed, which consisted in exposing birds to the influence of a freezing mixture, first in February, and afterwards in July and August, and observing in what degree they were cooled by remaining in this situation for equal lengths of time; the result was, that the same kind of animal was cooled six or eight times as much in the summer as in the winter months.⁷ This principle he supposes to be of great importance in maintaining the regularity of the temperature at the different seasons, even more so than evaporation, the influence of which, in this respect, he conceives has been much exaggerated.⁸ It would appear, however, that the sudden application of a high or a low temperature has a different effect upon

⁷ De l'Influence, &c. par. 3. chap. 3.

⁸ Ibid. p. 486, 7.

the power of generating heat from its more gradual and long continued application; in this case the heat is abstracted more rapidly from the animals after they have been exposed to cold, and less so after they have been exposed to heat.⁹ Hence we may conjecture that in sudden and great elevations of temperature, the great agent is evaporation, while in that progressive increase and decrease which depends upon the change of the seasons, the equalization of temperature is effected, at least, to a certain extent, by an alteration in the power of producing heat.¹

The result of our inquiry into the subject of animal heat has brought us to the conclusion, that it is the

⁹ De l'Influence, &c. par. 4. chap. 3, 4.

¹ I may remark that the observations of Dr. Edwards upon the effect of the long continued application of heat and cold, afford a general confirmation of the statements formerly made by Crawford and Lavoisier, and more lately by Dr. Delaroche, respecting the different degree in which the chemical state of the air is affected by respiration at different temperatures. They had indeed no idea of the difference which appears to result from the mode of applying the temperature, which, as we have seen, is so great as absolutely to reverse the effect; but so far as the conclusion from their experiments is concerned, it coincides with what may be deduced from Dr. Edwards's. In considering the nature of the operation by which the body is cooled, it is always necessary to bear in mind, that it is effected, under ordinary circumstances, in two ways, by a diminution of the process by which heat is evolved, and by the absolute abstraction of heat. Before quitting this topic, I may remark, that M. Magendie, in referring to the experiments of Dr. Delaroche, observes, "point de doute que l'évaporation cutanée et pulmonaires ne soit la cause pour laquelle l'homme et les animaux résistent à une forte chaleur." *Physiol.* t. ii, p. 403.

immediate effect of respiration, and that the lungs are the apparatus by which the heat of the system is evolved, and its temperature regulated. This is accomplished by the discharge of carbon and water, the first depending upon a chemical combination of the oxygen of the atmosphere with a portion of carbonaceous matter derived from the blood, during which combination heat is necessarily extricated ; the second upon the abstraction of a portion of the heat thus extricated in consequence of the evaporation of water from the surface of the pulmonary cavities.

Hence we obtain an answer to the second and third questions which were proposed at the commencement of this chapter, by what means is the uniformity of the temperature preserved under different circumstances? and how is the body cooled when exposed to temperatures higher than itself? With respect to the first point, it appears probable, that there is a provision in the lungs, by means of the combination of carbon and oxygen, for the production of the greatest quantity of caloric that can, at any time, be required for the wants of the system ; that when a less evolution of heat is necessary, this is partly effected by a diminution in the quantity of oxygen and carbon combined, but that it is principally brought about by the absorption of heat, in consequence of the evaporation of water, a process which is probably at all times going forwards, but which is increased at high temperatures ; and so far in proportion to the temperature, that, within certain limits, it can prevent the undue accumulation, or carry off

the excess of caloric, and prevent the body from acquiring a temperature beyond that which is natural to it.

The lungs are materially assisted in this cooling process by the perspiration from the skin, which, like that from the pulmonary vesicles, increases in the ratio of the temperature, and serves still more effectually to abstract the excess of caloric. And I may farther observe, that not only is the pulmonary and cutaneous evaporation immediately increased by whatever raises the temperature, but that the same circumstances provide for the production of a still greater effect, by increasing the quantity of the evaporable matter in consequence of the circulation being quickened, and all the secretions being proportionally augmented.

It would appear that we are not yet in possession of any facts which can enable us to compare accurately the quantity of carbonic acid formed, and of water evaporated, with either the heat generated on the one hand, or with what is absorbed on the other, nor to ascertain the proportional operation of the skin and the lungs. It seems, however, not an unfair assumption to regard them as adequate to the supposed effects, because we not only know that these processes are respectively the cause of the increase and diminution of temperature in bodies that are exposed to their influence, but because we find that they possess in themselves a provision for regulating the temperature of the living system, which would be altogether useless, were it not employed for this specific purpose.

I have endeavoured to show, in the preceding chapter, that this removal of carbon from the blood, is an operation essential to the well-being of the animal, independently of its effects in generating heat, and it is not improbable that the evaporation of the water may serve some other useful purpose, besides that of cooling the body at high temperatures. We may conclude, upon the whole, that these two functions, that of respiration and that of calorification, are strictly connected together, and mutually contribute, not only to the due performance of each other, but likewise to the integrity and perfection of the whole animal œconomy.

Having thus attempted to account for the operations by which the temperature of the body is regulated, it is necessary to consider how far they are connected with the other vital actions. The source of the heat which is evolved in the lungs would appear to be a proper chemical combination, of essentially the same nature with the combustion of charcoal. Now in this case there does not appear to be any thing absolutely necessary more than merely bringing the substances that are to act upon each other into sufficiently extensive approximation, which is effected by the physical structure of the lungs, and the mechanical action of the parts connected with the thorax. By the conjoined operations of digestion, secretion, excretion, and the other functions which affect the constitution of the blood, this fluid is brought into a proper state to be acted upon by the air; by the contractile power of the heart and the

capillaries, it is propelled into the appropriate organs, and while it is in this situation, the air is allowed to act upon it. The only direct vital action in this case would appear to be the contraction of the muscular fibre, at the same time that various chemical changes are brought about in the nature of the blood, which indirectly co-operate to effect the ultimate object. In the same manner we shall find that the production of cold in the lungs depends directly upon mechanical, and indirectly upon chemical causes, the air being mechanically impelled into the lungs, by the intervention of the contractility of the muscular organs connected with the chest, while a quantity of a secreted substance is provided, the watery part of which is dissolved by, or diffused through the air, and, as in other cases of evaporation, produces cold. In the same manner, or even by a more simple process, is the cutaneous evaporation effected; for here nothing more is necessary than for the secretory arteries to pour out the fluid through their capillary extremities, which is immediately carried off by the air that is in contact with the body.

Although in the actual state of the system it is impossible to conceive of such a successive train of operations, without the intervention of the nervous influence, yet this intervention would appear to be rather of an indirect or incidental kind, than one which is directly essential to the effect produced. How far the nervous agency is concerned in digestion and secretion will be considered hereafter, but supposing the blood and the different fluids to be properly

constituted, there seems to be nothing farther necessary, than that the former should be conveyed through the pulmonary vessels, and the latter poured out on its proper surfaces, while the air is duly impelled into the vesicles, for the carbonic acid and aqueous vapour to be generated. The nice adjustment of these operations to each other, and to the demands of the system, affords an example of that admirable adaptation of contrivance, which pervades every part of the animal oeconomy, but in which no powers or principles of action are concerned essentially different from those which are exerted in the other parts of the living system. It is indeed to this simplicity of action, or to the complicated results which are brought about by the operation of a few general principles only, that the perfection of the animal machine is mainly owing, in which it so greatly surpasses the most ingenious of human contrivances, and which more especially renders it a subject of our never-ceasing wonder and admiration.²

That the nervous system is indirectly concerned in the production and regulation of animal temperature appears, however, to be sufficiently proved by various observations and experiments. Legallois, although he found so exact a correspondence between the chemical effects of respiration and the degree of heat extricated, still conceives that the animal

² *Mechanice organum id laudat, ejusque auctorem celebrat sapientissimum, quod quæsito effectui producendo aptissimum, simulque inter omnia, quæ eundem præstare possent, simplicissimum est. Boerhaave, Oratio de Usu Ratioc. Mech. p. 14.*

temperature is very much under the influence of the nervous system, so as to lead him to conclude that whatever weakens the nervous power, proportionally diminishes the power of producing heat.³ We may likewise draw the same inference from the experiments of Mr. Brodie, and from the pathological observations of Mr. Earle and others of a similar kind. This is also the necessary deduction from the experiments of Dr. Philip, in which it appeared very clearly that the nervous influence is so intimately connected with the power of evolving heat as to lead him to conclude, as we remarked above, that the nervous power is a necessary intermedium between the different steps of the operation. In consequence of his discovery of the evolution of heat by galvanism from arterial blood, he regards the process of calorification as a case of secretion, and explains it upon his general principle of the identity of the nervous and galvanic influence, and of this influence being essential to the function of secretion.⁴ Although it

³ The destruction of a part of the spinal cord was found to diminish the temperature of an animal, and this, so far as appeared, without the disturbance of any other function; Philip's *Inq.* c. 7. § 2; *Ex.* 55, 6. p. 159, O.; *Quart. Journ.* v. 8. p. 75, 6.

⁴ See *Inquiry*, ch. 8. In connexion with the nervous hypothesis of animal heat, I may mention the singular speculation of Peart; he supposes that "animal heat is a compound, produced by a mixture of the nervous fluid with pure air;" *On Animal Heat*, p. 80. He farther informs us, that "the nervous fluid is composed of an earth, united with much phlogiston, and a quantity of ether," p. 111; and that "a quantity of this phlogiston of the nervous fluid unites with the ether of the pure air,

may, perhaps, be considered as merely a verbal inaccuracy, yet I should object to applying the term secretion to the extrication of heat, when the expression is intimately connected with the establishment of an hypothesis. Nor do I consider Dr. Philip's experiments, in the accuracy of which I place full confidence, to be sufficiently numerous or varied to enable us to draw so important a conclusion from them as that galvanism is an essential agent in the production of animal heat. The legitimate inference from them appears to be, that whatever be the chemical action of galvanism, it has the property of evolving heat from arterial blood, but I presume that the intimate nature of this operation is still unknown. Nor do I think that we are able to explain the facts of a more general nature which have been brought forwards by Dr. Philip and other physiologists respecting the connexion between the nervous influence and the production of heat.⁵ I

sufficient to saturate it, and form that heat which is called animal heat;" p. 112.

⁵ Hunter, *Anim. Econ.* p. 104, remarks, that the power of generating heat cannot "depend upon the nervous system, for it is found in animals that have no brain or nerves." See also *Phil. Trans.* for 1775, p. 457. Many of the facts brought forward in this paper, are curious and interesting, but the author totally fails in establishing his conclusion, that animal heat immediately results from the action of the vital principle, in consequence of his having overlooked many of the accompanying phenomena. An hypothesis has been lately advanced by Sir E. Home, respecting the production of animal heat, according to which it is restricted to the ganglionic part of the

conceive the most probable supposition to be that the capillary arteries are the part most immediately con-

nervous system. The doctrine especially rests upon the two positions, that there are certain animals which possess a brain, or some part equivalent to it, but whose temperature is not higher than that of the surrounding medium, while, on the other hand, all the animals that evolve heat are provided with ganglia. The most important facts that are brought forward in this paper are those on the comparative temperature of the two horns of a young deer, in one of which the nerves had been divided, the other being left entire. After some hours the divided horn had its temperature considerably diminished, but in five days it nearly recovered its natural state. The part was examined after the death of the animal, when it was found that "no union had taken place between the divided trunk," the natural conclusion from which, I conceive, to be that the decrease of temperature did not depend upon the division of the nerves, but upon some other circumstance attendant upon the operation. The author, however, formed a contrary conclusion; he remarks, that "it was evident from the recovery of its heat, that some other connexion had been formed between the nerves of the horn and those of the head." I may further observe, that I do not perceive how this experiment bears upon the peculiar hypothesis of the author, respecting the specific effect of the ganglionic nerves in generating animal heat; *Phil. Trans. for 1825, p. 257. et seq.*

Jan. 1826. I learn from the number of the Quarterly Journal of this month, vol. xx. p. 307, that Sir Ev. Home has repeated the experiment on the horn of the deer, and found, as before, that its temperature is lowered by the division of the nerve; but that in eight or ten days, the effect begins to diminish and ultimately ceases. The nerve was then examined, and the divided parts were found to be connected by a newly formed substance; thus, as the author conceives, accounting for the loss of temperature in the first instance, and for its subsequent restoration.

cerned, both because these are the organs in which the evolution of heat actually takes place, and because they are obviously under the influence of the nervous system.⁶

⁶ See Blumenbach, *Inst. Physiol.* § 168. et seq.

CHAPTER IX.

OF SECRETION.

WE have now gone through the functions which are more directly essential to the mere continuance of vitality, the circulation and the respiration, functions which cannot be suspended, even for a very short interval of time, without the immediate extinction of life, or a serious derangement of its more important actions. We have now to consider a second order of functions, which are absolutely necessary for our continued existence, but which would appear to be exercised only at certain periods, either when circumstances admit of their action, or, if we regard their final cause, when there is a demand for them in order to supply the wants of the system.¹ These are the three functions of secretion, digestion, and absorption. The first affords the means by which certain parts of the blood are separated from the mass, either to serve some useful purpose after their separation, or to remove some substance which is superfluous or injurious. By the function of digestion the aliment taken into the stomach experiences a series of changes in its constitution and properties, probably by the intervention of certain

¹ The old division of the functions into vital and natural, essentially depends upon this principle; but there was some inaccuracy in the mode of applying it, while the nomenclature is decidedly inappropriate.

secreted fluids, which converts it into the substance that seems to be the immediate source of nutrition, while, by means of absorption, the substance thus elaborated is carried from the digestive organs into the blood, where it becomes assimilated to this fluid, and is then transferred to every part of the system, dispensing life and heat, and affording materials for the formation of all the solids and fluids which compose the great machine.

It is obvious that the two former of these functions are so connected together, that it is impossible to give an account of one of them, without presuming upon a certain acquaintance with the other. The secretions cannot be formed until the blood has been already elaborated by the digestive and assimilating processes, while digestion, in its turn, cannot be effected until the stomach has secreted the gastric juice, which is the immediate agent in converting aliment into the materials of the blood. Upon the whole, however, it appears more convenient to begin by considering the function of secretion, as we shall, by this means, be better enabled to judge of the merits of the different hypotheses that have been formed to account for the operations of the digestive organs.

In considering the subject of secretion, I shall begin with a few preliminary observations on the nature of the function and on the structure of the secretory organs; I shall next give a brief account of some of the more important of the secreted substances, and shall endeavour to form an arrangement of them.

I shall, at the same time, inquire into the mode of their formation considered individually, and shall conclude with the various hypotheses that have been formed to explain the operation.

§ 1. *Description of the Organs of Secretion.*

The term secretion, according to its original and primary meaning, is equivalent to separation, and it would appear that this was likewise the technical sense in which it was used by the ancient physiologists, and by the earlier of the moderns, who, for the most part, conceived that the secretions previously existed in the blood, and were merely separated from it, either by mechanical means, or by certain chemical operations, somewhat analogous to precipitation. At present we generally attach a different meaning to the term, and conceive of it as essentially consisting in the production of some change in the secreted substance, either of a physical or chemical nature, proceeding upon the supposition that it did not previously exist in the blood.² Perhaps, however, it

² The late experiments of MM. Prevost and Dumas, where urea was detected in the blood, after the extirpation of the kidney, may, indeed, lead us to the former opinion; they will be more fully considered in a subsequent page. M. Magendie's definition may appear to favour the idea of mere separation. "On donne le nom generique de secretion à ce phenomène par lequel une partie du sang s'échappe des organes de la circulation pour se répandre au dehors ou au dedans; soit en conservant ses propriétés chimiques, soit après que ses élémens ont éprouvés un autre ordre des combinaisons;" *Physiol. t. ii. p. 343.* Haller

would be more correct to combine both these ideas in our conception of the process of secretion, and to define it, that function by which a substance is separated from the blood, either with or without experiencing any change during its separation.

But although it may be found convenient, or even technically correct, to extend the term secretion to both these classes of substances, it may still be proper to employ this difference, as the foundation for a subdivision of the secretions, into such as are simply separated, and such as are actually formed, as it may be presumed, that the separation of the former and the production of the latter will depend upon essentially different operations.

Secretion, in the same manner with all the other operations of the animal œconomy, may be considered as consisting in a vital process, operating through the intervention of certain physical powers; those which we suppose to be concerned in secretion are both mechanical and chemical; the mechanical means em-

simply defines secretion to be "*ea corporis animati functio, qua de communi sanguinis massa; alii, et a sanguine diversi, et a se ipsis varii, humores ea lege parantur, ut in qualibet ejus corporis particula idem constanter humor generetur.*" *El. Phys.* vii. 1. 1. It may be objected to this definition, that the latter clause prevents our admitting those varieties of action, which not unfrequently take place in the same organ, in consequence of different circumstances, both external and internal. Cullen's section on secretion in his "*Institutions*," § 275. 285. besides its other merits, is valuable as showing into how small a compass what was really known respecting this function, at that period, might be comprised.

ployed are usually conceived to be something analogous to filtration or transudation, where a portion of a compound is separated from the remainder, in consequence of the minuteness of its particles enabling it to pass through orifices or pores, which will not allow of the larger particles being transmitted. It is not, however, impossible, that a substance which previously existed in the blood, may be separated by a chemical operation, although, in this case, we may generally suspect that the substance separated will have its constitution altered. In the case of those substances which are actually formed, not having previously existed in the blood, it is extremely probable, although not absolutely necessary, that something more than a mere mechanical operation must have taken place; and when a new chemical compound is formed, we may reasonably infer, that it must have been produced by the intervention of a chemical agent. These agents may be of two kinds; they may be either extraneous bodies introduced into the blood, and acting upon some of its elements, or they may consist of some of the constituents of the blood acting upon the other parts of this fluid. We shall find, in our examination of the different secretions, that it is often very difficult to ascertain, in which of these two operations the substance in question may originate, or what is the exact nature of the action, even where we have reason to conceive that we know to what class of operations it should be referred. It may be assumed as a general principle, that those secretions

which are formed by mere transudation are produced by a much more simple apparatus than those of the other class ; and, indeed, in the greatest number of cases, we are not aware of the existence of any appropriate structure. Hence it follows that the secretory organs which possess a complicated fabric, belong almost exclusively to that class where the substance did not previously exist in the blood, but is actually generated by the process of secretion.

In those cases where we have an organ possessing what is considered as the most perfect structure, or one which consists of the greatest variety of distinct parts, we find a body, which is more or less of a rounded form, and has hence obtained the name of gland. When we examine the intimate structure of glands, they appear to be composed of a number of small arteries, which ramify in various directions through a mass of cellular texture. A part of the blood is returned by corresponding veins, but we also find another set of vessels, containing the secreted substance, which generally unite into one or more excretory ducts. The gland usually consists of a number of rounded bodies, which may be detached from each other, and compose what are termed lobes ; these are again divisible into smaller and smaller lobes, until we at length arrive at the smallest parts into which it is possible to subdivide them, which have obtained the name of acini. The glands are also provided with nerves, but it is a question which will be discussed hereafter, how far

the nervous influence is essential to secretion, or in what way it operates.³

There is considerable obscurity respecting the intimate structure of glands, and particularly, whether any specific organ intervenes between the secreting arteries and the excretory ducts. Malpighi, who was one of the first anatomists that attended to the minute structure of glands, supposed that there was, in all cases, a cavity or follicle, as it was termed, which was the immediate organ of secretion. Ruysch, on the contrary, who bestowed much care on the investigation of this point, and who particularly excelled in the art of injecting minute vessels, conceived that he had disproved the doctrine of Malpighi, and concluded that there is no intervening organ, but

³ For an account of the structure of glands, I may select the following authors, as those who have given a summary of the more modern discoveries and opinions; Cheselden, *Anatomy*, p. 146, 7; Winslow, *Anat. sect.* 10. § 575. et seq.; Boerhaave, *Prælect.* t. ii. § 240. et seq.; his account is more physiological than anatomical; Haller, *El. Phys.* vii. 2.; Sabatier, *Anat.* t. iii. p. 342. et seq.; Dumas, *Physiol.* t. ii. p. 20, 1; Bichat's section, "systeme glanduleux," *Anat. Gen.* t. ii. p. 598. et seq. contains much that is ingenious, but as usual, in some parts, is hypothetical, and the expressions obscure and mystical; Blumenbach, *Inst. Phys.* § 468. et seq.; Monro (3^d), *Elem.* v. ii. c. 3.; the article "gland," in Rees's *Cyclop.*, may be perused with advantage. Nuck's elaborate treatise entitled "*Adenologia*," being especially intended to illustrate the structure of the lymphatic glands, will be more properly considered in a subsequent chapter; I may, however, remark in this place, that he commences by giving us a list of all the proper secretory glands, which are found in the body, amounting to 53 different sets, or attached to as many different organs.

that the artery immediately terminates in the duct, so as to show that the operation of secretion must take place in the artery itself. It is upon the whole probable that neither of the opinions is absolutely correct, but that different glands possess different structures in this respect, and that upon the presence or absence of the follicle, a part of the specific effect produced by the different glands may depend.⁴

⁴ This controversy occupied the attention of the most learned anatomists and physiologists during the latter part of the 17th, and the commencement of the 18th centuries. Malpighi seems to have first published his opinion in the year 1665; it was pretty generally adopted, until Ruysch called it in question in 1696. Malpighi is admitted to have been one of the most learned and correct anatomists of his age, and particularly excelled in the investigation of the minute structure of parts. Haller thus describes Ruysch; "etsi neque ingenii velocitate valde eminuit, neque assiduitati legendi, aut eruditione, frequentissima tamen cadaverum consuetudine, et opportunitate ad consulendam naturam, et dissectionibus penè per totos octaginta annos continuatis, et artifice etiam manu, plerumque mortales superavit, eoque majorem auctoritatem sibi comparavit, quod ab hypothesi alienior, parum ultra ea doceret, quæ viderat." *El. Phys.* vii. 2. 8. We have a very full detail of the successive steps of the discussion in the *El. Phys.* vii. 2. 6. . 14; Haller entitles the last of these sections, "causa Ruyschiana vincit." See also Boerhaave, *Præl.* § 247, 257. cum notis. Those who are disposed to consult Malpighi and Ruysch in the original, may find a detail of their opinions and discoveries in Malpighi, *Exerc. de Struct. Viscer.* cap. 2, 3. containing an account of the liver; *Epist. ad Reg. Soc. Lond. de Struct. Gland.* written in 1688, and in *Op. post.* p. 101. Ruysch's account of his opinions is contained in his *Epistle to Boerhaave de Fab. Gland.* He informs us, p. 65, that at a very early period of his life he examined the structure of the mesenteric glands by means of fine tubes, that were made by Musschen-

It is necessary to remark, that it is comparatively in but a few instances, that we meet with a complete glandular apparatus; in some cases the proper glandular structure appears to be altogether wanting; in some we have a pouch, or cavity, in which the secretion is lodged, being with

broek, through which he inflated them with air from the lacteals, in p. 81, we have a small plate of the mesenteric vessels and the connected glands, composed of their minute terminations. For the particular illustrations which he gives of his opinions; see Epist. probl. 4 ad Campdomercum, de liene, p. 6; Epist. probl. ad Grætz, de mam. struct. &c. p. 7; Adver. Anat. dec. 1. § 4. p. 13; Thes. Anat. 6. No. 3. not. 2. p. 18. on the small glands of the nose; No. 33. not. 1. p. 28. on the glands of the stomach; No. 73. p. 38. on the cortical substance of the brain; Thes. Anat. 8. No. 34. p. 13, 4. in describing the vessels of the placenta, he takes occasion to state his sentiments respecting the structure of glands generally; Thes. Anat. 10. No. 61. p. 14. on the mesenteric glands; Thes. Anat. max. No. 118, 9. p. 17, 8. on the glands of the mesentery and stomach. Boerhaave's Epistle to Ruysch, in answer to the one referred to above, is a very interesting specimen of the learning and candour of the writer. In p. 36. he acknowledges his conviction of the force of Ruysch's reasoning. "Fateor," he says, "hoc tam forte mihi videri, ut fere nesciam, an effugere quis possit vim hujus argumenti." He refers to an experiment in which an injection had been passed directly from the vena portæ into the pori hepatici, without passing through any intervening follicle. Again, in p. 37, he observes, "Ars itaque tua certo nos docet, in multis, forte et in omnibus, corporis humani partibus ita disponi extremas canales ortas ab arteriis sanguiferis, ut directe, absque interposita glandulosa machina, desinant in apertos meatus &c." We have a perspicuous account of this controversy in Bell's Anat. v. iv. p. 16. et seq.

or without an excretory duct; while, in other instances, there would seem to be neither the cavity nor the excretory duct, but where the secreted substance is simply poured out upon the surface of a membrane, whence it is removed without any specific apparatus.⁵ There are likewise many examples, where a substance that did not previously exist in the blood is separated from it, and conveyed to its appropriate destination, but where we can perceive neither the organ by which it is produced, nor that by which it is afterwards deposited. It is also to be observed that in those cases where we are able to detect the secretory apparatus, its magnitude and structure, so far as we can judge, bear no relation to the change which is produced upon the matter secreted; we observe, for example, a substance, which apparently differs but little from the blood or any of its constituents, to be formed by a very complicated organ, while an organ, apparently of the most simple kind, is employed in producing a body of totally new properties.

Glands have been arranged in various ways, according to their anatomical structure, and their supposed physiological uses; but it may be doubted whether any real advantage has accrued from these arrangements, or whether there can be said to be any natural foundation for them. Thus the division of

⁵ Dr. Young enumerates the following among the different structures that are employed in secretion; exhalent vessels, tubular glands, conglomerated glands, follicles, pores, parenchymatous glands; Med. Lit. p. 110.

glands into conglobate and conglomerate,⁶ which was generally adopted by the older writers, the former consisting of only one lobe, the latter of a number of lobes, connected together by cellular substance, it would be difficult in many cases to adhere to, and would lead to no practical advantage. And the same observation may be made upon the division of glands into secretory and excretory, the former producing a substance which serves some immediate useful purpose in the system, while the object of the latter is to separate something from the blood which is useless or noxious, and is accordingly rejected as soon as it is formed. For though there are various bodies which are obviously intended for the first of these objects, and there are some which appear almost certainly to be confined to the latter, yet there are many which it is difficult to say in which division they ought to be placed, and some, perhaps, have an equal claim to either class, being, in the first instance, secreted from the blood as injurious to the system, and yet after their separation, and before they are finally discharged, serving some useful purpose. Probably the separation of carbon from the blood in the lungs may be a case of this double

⁶ The division of glands into conglomerate and conglobate appears to have been made by Sylvius; *Disput. Med.* 3. § 25, 6, 7. *Op.* p. 11; the former designating the more compounded structure, which is found in certain of the secreting organs, the more simple or conglobate glands being technically restricted to those which are connected with the lymphatic system. See *Haller El. Phys.* ii. 3. 16; vii. 2. 2.

action, where the excess of carbon in the system is positively noxious, at the same time that the process of separation may be so conducted, as to act in a very important manner upon the animal œconomy. Upon the whole then, it will appear, that there is no arrangement of the glands which can be of any considerable use in explaining the mode of their action, or in throwing any light upon the nature of the substances which they produce.

We shall probably find it almost as difficult to make any arrangements of the secretions themselves as the organs which produce them; yet before we proceed to consider the properties of so numerous a class of substances, it would be very convenient to be able to dispose them into groups or classes, the individuals composing each of which might bear some relation or have some reference to each other. One of the oldest arrangements of the secretions was into recrementitious and excrementitious, a division which corresponded to the secretory and excretory glands respectively, and of course liable to the same objections, besides its being neither sufficiently comprehensive nor sufficiently minute to be of any real utility. As the knowledge of physiological science advanced, other classifications were proposed, constructed upon more technical or scientific principles, generally referable either to the supposed chemical or mechanical properties of the substances, according to the tenets of those by whom they were respectively advanced. Haller adopted the chemical method, and classed the secretions under the four heads of aqueous,

mucous, gelatinous, and oily,⁷ a classification obviously too general to throw much light upon the nature of the substances concerned. Fourcroy also employed the chemical method, and made the more elaborate arrangement of the secretions into the eight classes of hydrogenated, oxygenated, carbonated, azotated, acid, saline, phosphated, and mixed.⁸ But although this arrangement was more scientific than any which had preceded it, and, in some measure, corresponded with the improved state of modern chemistry, it may be questioned whether it was not rather formed upon theoretical principles, than upon the actual nature of the substances concerned.

Before I attempt to form a new arrangement, there is a question to which I have already alluded, and upon which it will be necessary to decide, whether we are to consider those substances as secretions, which appear to exist ready formed in the blood, and which are separated from it, as far as we can perceive, without any appropriate or specific apparatus, such for example as the muscular fibre. This substance in all its chemical, and we may perhaps add, its mechanical properties, resembles the fibrine of the blood, and we cannot doubt that the fibrine is, by

⁷ *El. Phys.* vii. 1. 2.

⁸ *System*, v. ix. p. 159. Richerand gives us a different arrangement as the one proposed by Fourcroy, *Physiol.* p. 235; that in the text, being taken from his great systematic work, we may regard as the result of his more matured reflection; Richerand does not inform us in which of Fourcroy's works the arrangement is contained which he has adopted.

some means, separated from the mass of blood, and deposited in the situation in which we find it to exist, as a constituent of the muscles; yet we are altogether unable to trace the intermediate steps of the operation. The same kind of remark nearly applies to some of the fluids, especially those which are the result of morbid action. The various dropsical fluids, for instance, seem to have the same constitution with the serum, except in the proportion of water which they contain, and it would appear that they are separated from the blood by mere transudation through a membrane, a process analogous, or very similar to filtration. Upon the whole, I conceive it will be more convenient to regard all these substances as secretions, whatever may be their relation to the blood or to any of its constituents, and whatever we may conceive to have been the mode of their formation.

Another question of a nature not very different from the above, respects certain substances, which are found both in the blood and in some of the secretions, but concerning the origin of which there is great uncertainty, whether they are originally received into the stomach along with the aliment, and pass into the digestive organs without undergoing any change, whence they are taken into the blood and again separated from it, still, without any alteration; or whether there be a provision made for their formation in some part of the system. The doubt that arises on this point, depends principally upon the difficulty which we have in conceiving of any way by which

they could be produced from the elements that enter into the composition of the blood, as unless we admit of the operation or existence of new affinities, or affinities totally different from those which we observe in any other natural objects, we cannot suppose them to be actually generated in the system; at the same time there are perhaps equal, or even greater difficulties, in the supposition that they are introduced into the system *ab extra*. This question will more particularly fall under our consideration in the following section, where the evidence will be examined on which each opinion rests; at present I shall only remark, that it appears more convenient to consider all these substances as secretions, because in whatever manner they may be introduced into the blood, we find that they actually exist there, and are probably removed from it by the secretory organs.

I think there can be little doubt, that the only method of arranging the secretions, which can be of any use in giving us an insight into their nature, and the relation which they bear to the blood, must be founded upon their chemical composition; and although from our imperfect knowledge on this subject, such a mode of classification must be likewise necessarily imperfect, yet as there is no mode of arrangement to which a similar objection might not be urged, I shall not hesitate to make the attempt.

But there are certain technical difficulties which meet us at the outset. The secretions many of them consist of substances composed of a number of ingredients, possessed of different properties, where it may

not be easy to decide which ingredient predominates, or gives its peculiar qualities to the compound. We shall also find that if we examine a series of secreted fluids, we shall perceive that they all closely resemble each other in what may be termed their specific properties, yet that those at each extremity of the series may so far differ from that which was adopted as the standard or type of the rest, that the resemblance may be more nominal than real, and the difference may at length proceed so far, that they may even bear a nearer resemblance to some other class than to that in which they are placed. Another circumstance, which causes considerable embarrassment in a chemical arrangement of the secretions, is, that the same gland, in different states of the system, produces substances of a very different nature, and when the affection amounts to the degree which constitutes disease, entirely new substances are frequently formed, unlike any thing which previously existed, and which, although they must be regarded as altogether morbid effects, yet they are strictly entitled to the appellation of secretions, and which indeed, had we a complete knowledge of the subject, would form the most interesting object of our investigation, by making us acquainted with the nature of the morbid action which had taken place, and even the amount of the deviation from the standard of health. This, however, in the present state of our knowledge, we are quite unable to accomplish. In order to take a complete view of the subject, it would be necessary to examine the secretions in all their

various states, to mark the gradations from the healthy to the morbid condition, and to observe what new characters were assumed in each of them.

Taking into account all these circumstances, and bearing in mind that the present state of our knowledge is confessedly imperfect, it will be sufficiently evident, that any arrangement which I can propose must be necessarily incomplete; but I am not, on that account, deterred from making the attempt, because, if the method itself be fundamentally correct, even an imperfect view of it will be useful, by teaching us what parts require further elucidations, and instructing us in the best method of accomplishing it. The classes into which I propose to arrange the secretions, are the eight following; the aqueous, the albuminous, the mucous, the gelatinous, the fibrinous, the oleaginous, the resinous, and the saline.⁹

⁹ I shall insert in this place the arrangements of the secretions that have been proposed by some of the most eminent of the modern physiologists. Sabatier and Boyer, with most of the French anatomists, adopt the division of the secretions into recrementitious and excrementitious, to which they generally add an intermediate class. Boyer places the following among the recrementitious humours, as he styles them; blood, lymph, jelly, fibrous matter, fat, marrow, matter of internal perspiration (serous transudation), and the bony juice; among the excrementitious, the matter of insensible perspiration, sweat, discharge from the nose, ears, and eyes; and in the third or intermediate class, tears, saliva, milk, bile, pancreatic juice, and semen; Anat. t. i. p. 8, 9. Magendie divides them into exhalations, follicular secretions, and glandular secretions, but he candidly acknowledges the imperfection of his method; each of the classes is subdivided into numerous species; Physiol. t. ii.

§ 2. *Account of the Secretions.*

The first class of secretions, the aqueous, are those that consist almost entirely of water, where the properties of the substance depend upon its watery part, or when any other ingredient which it may contain is in

p. 343. et seq. Plenck divides the humours of the body into crude, sanguine, lymphatic, secreted, and excrementitious; the secreted fluids he arranges under the heads of milky, watery, mucous, albuminous, oily, and bilious; Hydrol. p. 31, 2. Richerand arranges them into the six classes of saline, oily, saponaceous, mucous, albuminous, and fibrinous; Physiol. § 88. p. 235. Blumenbach makes the following classification; milk, the aqueous fluids, the salivary, the mucous, the adipose, and the serous, while the semen and the bile are supposed to be substances sui generis; Inst. Phys. § 467. Berzelius adopts the old division into secretions and excretions, a division which is founded rather upon the final cause of their formation, than upon their properties, or the mode of their production; he remarks, however, that the secretions are all alkaline, while the excretions are acid, a remark which, I conceive, will scarcely be found to apply in all cases; Med. Chir. Tr. v. 3. p. 234. Dumas classes the secretions in four divisions, according to the more or less simple structure of the organs which produce them; those that are formed without any specific organ, by the most simple organ, by a gland, and by the complete secretory apparatus; Physiol. t. ii. p. 15. . 8. Dr. Young arranges the secreted fluids into the classes of aqueous, urinary, milky, albuminous, mucous, unctuous, and sebaceous; Med. Lit. p. 109.

The writer of the article "Anatomy" in Brewster's Encyclopedia, has drawn up the following arrangement of the secreting and excreting organs, with the fluids which they produce, which is valuable as pointing out the relation which exists between them. They are first divided into secreting surfaces and secreting organs. Of the surfaces we have three divisions;

too small a quantity to give it any specific characters. The only two secretions which fall under this class, are the cutaneous perspiration and the aqueous exhalation from the lungs. Of the cutaneous perspiration I have already given some account in the two last chapters, where I have stated, that under ordinary circumstances, a portion of water is exhaled from the

1. Those which separate matters already formed in the blood, viz. the serous, producing serum or coagulable lymph, and the cellular, producing serum and fat; 2. Those which separate from the blood matters that are little changed, viz. synovial membranes, forming synovia and mucous membranes, forming mucus; 3. Excreting surface, viz. the skin, giving out the matter of perspiration. The secreting glands are arranged under the four heads of such as are attached to the organs of sensation, those of digestion, those of reproduction, and glands that are partly secretory and partly excretory. Under the first head we have the papillæ of the tongue, which secrete a watery fluid, the ceruminous glands which secrete the ear wax, and the lachrymal which secrete the tears. Under the second head we have the parotid, submaxillary, and sublingual glands, which secrete saliva, the pancreas, which secretes its peculiar juice, the spleen, to which no secretion is assigned, and the liver, which produces the bile. Under the third head we have the testes, prostate gland, and the mammæ, which respectively secrete the seminal fluid, the prostatic fluid, and the milk; and under the fourth head, we have the kidneys, which produce the urine, and the renal glands, which produce a blackish fluid; vol. i. p. 830, 1. In addition to the eight classes of secretions which are enumerated above, I am disposed to think that we might with propriety admit a ninth class of aeriform fluids, of which the air in the swimming bladder of fishes may be adduced as an example; upon strictly technical principles, the air of expiration may be placed in the same division. See remarks by Dr. Baillie; Works by Wardrop, v. i. p. 69. et seq.

surface of the body in the form of an invisible vapour; but when its quantity is by any means increased, it assumes the state of a fluid, and is collected in drops on the skin. It does not appear that its chemical nature is different in these two states, although it is not very easy to decide absolutely on this point, in consequence of the difficulty of obtaining any quantity of it when in the state of vapour. The sensible perspiration may be procured in sufficiently large quantity, and has been examined by several eminent chemists, but except the water, it seems doubtful whether any of the ingredients that have been detected in it are essential to its nature. It appears probable that the perspiration differs considerably according to the states of the system, not only as affected by various morbid actions, but from internal causes, or the effect of internal agents upon it, and there is likewise reason to believe, that it may be habitually different in different individuals. Upon all these points, however, it must be confessed that we have no very accurate information, as the attention of those who have examined this substance has been almost exclusively confined to ascertain its quantity, and the pathological effects which have been supposed to be the result of its discharge from the system. There is, however, reason to suppose, that these have been very much exaggerated, and that many diseases which were conceived to depend upon the suppression of the cutaneous perspiration, are owing to an entirely different cause, of which the

peculiar condition of the skin was only one of the effects or symptoms.

There are many facts that appear to prove, that the skin emits a peculiar odorous matter, by means of which dogs, and other animals that possess a delicate scent, are enabled to detect the presence of other animals, or to trace them out for long distances. We have, perhaps, no decisive means of ascertaining whether this odorous effluvium depends upon the perspiration itself, or upon some other secretion which is mixed with it, and discharged from the body along with the perspirable matter. Upon the whole, however, it is probable that they are distinct substances, that the proper matter of perspiration is produced from every part of the surface, and is nearly or altogether without odour, while there are certain parts of the body which are provided with glands, that secrete a peculiar or specific substance, which composes the odoriferous effluvium. This latter would appear to be of an oily nature, and will therefore belong to a different class.

The perspirable matter, in the purest state in which we are able to procure it, seems to have been first examined by Berthollet,¹ and afterwards by Fourcroy,² but the most elaborate analysis is that of Thenard. He considers it to be essentially acid, and supposes that the acid is the acetic; it contains

¹ Journ. de Phys. t. xxviii. p. 275.

² System, v. ix. p. 280. et seq. He informs us that Vauquelin and he discovered urea and phosphate of lime in the perspiration of horses, p. 289.

an appreciable quantity of muriate of soda, and perhaps of potash, with traces of the earthy phosphates, and of oxide of iron; there also appears to be a very minute quantity of an animal matter.³ The matter of perspiration has been still more recently examined by Berzelius, and with results considerably different from Thenard's. He indeed supposes it to contain a free acid, but this he conceives to be the lactic, accompanied with the lactate of soda, together with the muriates of potash and soda, and a minute quantity of animal matter;⁴ it appeared, indeed, to be identical with the substance which Berzelius had announced as existing in the serosity of the blood, and many other of the animal fluids.

It may be reasonably doubted whether the aqueous exhalation from the lungs should be considered as an immediate secretion from the blood. I have already made some remarks upon its origin, and have stated that I conceive it, upon the whole, more probable that it proceeds merely from the aqueous part of the mucus, which is evaporated from the surface of the pulmonary vesicles, than that it is a distinct or separate secretion. So far as its chemical constitution has been examined, it appears to be the same with the cutaneous transpiration, and to consist of water, perhaps, holding in solution minute portions of saline

³ *Chimie*, t. iii. p. 712.

⁴ *Thomson's Ann.* v. ii. p. 415; *Med. Chir. Tr.* v. iii. p. 256, 7; *Ann. Chim.* t. lxxxix. p. 20. See also *Thomson's Chem.* v. iv. p. 547. et seq.; *Henry's Elem.* v. ii. p. 434; *Ure's Dict. Art.* "Sweat."

or animal matter, but we have no very certain information respecting either their quantity or exact nature.⁵

The second class of secretions, the albuminous, constitute a very numerous and important series of substances, some of which are in the solid, and others in the fluid form. All the membranous, or white parts of animals, as they have been termed, consist essentially of albumen, which appears, from the experiments of Mr. Hatchett, to differ from the albumen of the blood only in being detached from the greatest part of the extraneous matter with which it was united, and in being in a coagulated state.⁶ We have also a considerable number of fluid albuminous secretions; the surfaces of all the close cavities of the body, such as the thorax, the abdomen, the pericardium, the ventricles of the brain, and even the interstices of the cellular substance, are continually secreting a fluid, which seems to differ from the serum of the blood principally in containing a much smaller quantity of albumen. There are many morbid conditions of the body, in which these fluids become præternaturally increased in quantity, sometimes to a great extent, so that we have an oppor-

⁵ We are informed by M. Magendie, in his *Mem. on Transpiration*, that M. Chaussier has proved that the vapour from the lungs contains a quantity of animal matter, by keeping a portion of it in a close vessel exposed to an elevated temperature; upon opening the vessel a very evident putrid odour was exhaled from it; p. 16.

⁶ *Phil. Trans.* for 1800, p. 399. et alibi. See vol. i. of this treatise, p. 43. et seq.

tunity of examining them with great accuracy. Many chemists, both on the continent, and in this country, have applied themselves to this investigation, and it is clearly ascertained, that they consist of a certain quantity of albumen, which may be regarded as what gives them their essential character, of another animal matter similar to that found in the serosity of the blood, and of the same neutral and earthy salts which we find in that fluid. It would appear that the salts and the additional animal matter are nearly in the same proportion in all cases, while the proportion of the albumen is varied from a quantity nearly equal to that in the serum of the blood, to one almost too small to be recognized even by the most delicate tests.⁷ This, therefore, affords an instance of that inconsistency, to which all attempts at arrangement are liable, where we place a secretion in the class of albuminous, although the smallest quantity only of albumen enters into its composition.

The morbid albuminous fluids, which we have the most frequent opportunities of examining, are those from the abdomen, from the ventricles of the brain, from the pericardium, from the cavity of the spine, from that of the testicle, and from the cellular texture generally.⁸ As a general rule, the fluid from

⁷ Berzelius, *Ann. Phil.* v. ii. p. 384, 5, and *Med. Chir. Tr.* v. iii. p. 251. et seq.; Henry's *Elem.* v. ii. p. 431, 2; Thomson, *Chem.* v. iv. p. 528; Thenard, *Traité*, t. iii. p. 683, 4. p. 686, 7; Magendie, *Physiol.* t. ii. p. 344. . 6.

⁸ Marcet, in *Med. Chir. Tr.* v. ii. p. 340. et seq.; Bostock, in ditto, v. iv. p. 53. et seq.

the cavity of the abdomen contains the greatest proportion of albumen, and that from the brain, the least, but there are many exceptions to it.⁹ We also find that the fluid from the same part contains more or less of the animal matter according to the states of the constitution, the rapidity with which the deposition of matter is made, the length of time in which the fluid has remained in the cavity, and probably from other circumstances; but I do not find that we are able to lay down any general principles which are applicable in all cases. By comparing together a considerable number of experiments, which I have performed at different times on fluids of this description, I have been led to conceive, that the variation in the quantity of the albumen is much greater than of the other ingredients, so that while in certain of these fluids, as for example, in that of hydrocephalus, it is not more than one-third or one-

⁹ In a very remarkable case of chronic hydrocephalus, which occurred lately in Guy's Hospital, the fluid was not only in extraordinary quantity, but contained an unusually large proportion of solid contents. I examined a portion of it, with which I was favoured by Mr. Aston Key, and found the proportion, both of the animal and of the saline ingredients, to be very much more than is usually present in fluids of this description, so as to be nearly double the average quantity. How far this peculiarity belongs generally to the disease in its chronic form, is a question which, I believe, the present state of our knowledge does not enable us to answer. It is much to be desired, that the particulars of so very curious a case may be given to the public, by some of the gentlemen who assisted in the examination.

fourth of what it is in the fluid from ascites, the quantity of the saline contents, and of the uncoagulable animal matter are nearly the same.¹ Nor is it in its quantity or proportion alone that the saline matter of these fluids resembles that of the blood; in various instances where it has been examined, it would appear to be similar to it in its composition, and particularly in the circumstance of its containing uncombined soda. This salt is so generally found in those fluids, which, in other respects, exhibit the albuminous properties, that it would appear to be in some way necessarily connected with, or essential to them, while, in the fluids possessing other physical characters, and which do not contain albumen, we never find any indications of a free alkali.² With respect to the formation of the albuminous secretions, we have no knowledge of any appropriate organ by which they are produced; and as they so exactly resemble the serum of the blood, except in the proportion which there is between the water and the solid contents, it may be fairly questioned, whether

¹ See the paper referred to above, and more particularly the synoptical table, p. 73.

² Mr. Brande, proceeding partly upon this circumstance and partly upon his discovery of the effect of the galvanic apparatus in coagulating albumen, considers it, while in the liquid state, as essentially a solution of this peculiar animal matter in alkali, and attributes its coagulation to the subtraction of this alkali by the negative end of the interrupted circuit; *Phil. Trans.* for 1809, p. 377. I have already given my reasons for conceiving that this hypothesis cannot be maintained; *Med. Chir. Trans.* v. ii. p. 173, 4.

they are not formed simply by filtration or transudation; still, however, according to the view which I have taken of the nature of secretion, this will not exclude them from the list of secreted substances.

We now come to the third class of secretions, the mucous, which differ from the aqueous and the albuminous in this essential particular, that whereas the two former appear to consist of substances that are merely separated from the blood, the essential character of the mucus depends upon a substance, which did not exist in the blood, but which is formed by the action of the gland. We accordingly find, that in the case of these secretions, we are generally able to demonstrate the organ by which they are produced, and that some of the mucous glands are among the most elaborate with which the body is furnished.³ The mucous secretions are distinguished by their viscosity, or their capacity of being drawn out into threads, and by being with difficulty soluble in water, although they are already united to a considerable quantity of it. The animal matter which forms the basis of the mucous secretions, and which gives them their essential characters, appears in many of its chemical relations to resemble albumen in the

³ Bichat considers the fluids produced from serous membranes as only exhalations, while those from mucous membranes are properly secretions; *Traité des Membranes*, p. 5, 6. Magendie also, who makes a distinction between exhalations and glandular secretions, places some, at least, of the mucous fluids in the latter class; *Physiol.* t. ii. p. 360. et seq. According to the test proposed by Berzelius, see above, p. 330, the mucous fluids held a kind of intermediate rank between the secretions and the excretions.

coagulated state, so that we are led to suppose, that at least one effect of the mucous glands consists in the coagulation of the albumen of the blood.⁴ Another circumstance which characterizes the mucous secretions, and especially distinguishes them from the albuminous, is the nature of the salts which they contain, for whereas the latter are very similar to those in the serum, and resemble it in containing uncombined soda, the salts in the mucous secretions are in the neutral state.⁵

The mucous secretions differ from the albuminous in their seat, as well as in their composition; the albuminous are lodged in the close cavities of the body, while the mucous are always found in those cavities or passages that have a communication with the atmosphere; such as the mouth, the nose, the oesophagus, the stomach, the alimentary canal, the bladder, the trachæa, and the air vesicles of the lungs. In many of these parts we find a glandular apparatus, which is often peculiarly large and elaborate in its construction, as for example, in the gland which secretes the saliva; but there are other cases, where a substance that appears very nearly to resemble the saliva, is formed without the intervention of any glands that we are able to detect.

All the mucous membranes, as they are termed,

⁴ Ed. Med. Journ. v. ii. p. 44, 5; Nicholson's Journ. v. xiv. p. 149.

⁵ Mr. Brande informs us that the saliva is not alkaline; he finds, however, that the galvanic apparatus separates albumen from it in the coagulated state, as it does from the alkaline serous secretions; Phil. Trans. for 1809, p. 374. et seq.

secrete a fluid which appears to be nearly similar in the various parts of the body; and to the same class we refer the saliva,⁶ and the gastric juice,⁷ although

⁶ As the saliva may be easily procured in considerable quantity, it has been frequently made the subject of chemical analysis. An account of what had been done on the subject by the earlier physiologists will be found in Boerhaave; *Prælect.* t. i. § 66; and in Haller, *El. Phys.* xviii. 2. 10. A correct detail of the modern discoveries is contained in the *Systems* of Dr. Thomson, v. iv. p. 515. et seq.; and Dr. Henry, v. ii. p. 409. In some experiments which I performed on saliva in the year 1805, I conceived that I had detected in it two kinds of animal matter, one composing the soft masses, and giving it its consistence and physical characters, nearly similar to coagulated albumen, the other dissolved in the water of the saliva, along with the salts, and resembling the serosity of the blood; *Ed. Med. Journ.* v. ii. p. 44, 5; and *Nicholson's Journ.* v. xiv. p. 149. Dr. Thomson agrees with me in thinking that the former of these substances exhibits the properties of coagulated albumen; *System*, v. iv. p. 517. Berzelius has more lately examined saliva; he also supposes that it contains two kinds of animal matter; the one which he styles mucus appears to be the same with what I conceive to be coagulated albumen; he likewise supposes that there is a relation between some of the component parts of the saliva, and the serosity of the blood, although not of that nature which I had announced. His analysis of the saliva is as follows:

Water	992.9
Peculiar animal matter	2.9
Mucus	1.4
Alkaline muriates	1.7
Lactate of soda and animal matter	0.9
Pure soda	0.2
	<hr/>
	1000.0

Ann. Phil. v. ii. p. 379, 0; *Med. Chir. Tr.* v. iii. p. 242, .4; *View of Animal Chem.* p. 61, 2.

I shall refer those of my readers who take an interest in trac-

we can scarcely conceive, but that this latter must contain some ingredient besides its mucous part, to which it owes its peculiar property of acting upon the aliment taken into the stomach: but this point will be more fully considered in the next chapter.

It is somewhat doubtful whether the tears should be referred to the class of albuminous or of mucous fluids. An analysis was made of them by Foureroy and Vauquelin, which, considering the period when it was performed, must be regarded as very accurate; and as from this we learn that they contain an uncombined alkali, we might be induced to place them among the albuminous secretions. It appears, however, that besides albumen, some other animal matter enters into their composition, which gives them their specific properties, and which, so far as we can form an opinion from the detail of the experiments, resembles

ing the progress of knowledge, to the account of the saliva which is given by Baglivi; he examined the action of various chemical re-agents upon it with considerable accuracy, and discusses with much ingenuity, although not always very correctly, its supposed effect in the process of digestion; Dissert. 2. Circa Salivam; Op. p. 412. et seq.

7 The accounts which had been generally given of the gastric juice were that it possessed no properties which were not common to all the mucous secretions, and especially that it was neither acid nor alkaline. We have, indeed, occasional observations by different chemists, who asserted that they had detected an uncombined acid in it, but this was supposed to be accidental, or to be owing to some morbid cause, until Dr. Prout announced that, during digestion, the contents of the stomach were essentially acid, and that this acid was the muriatic; Phil. Trans. for 1824, p. 45. et seq.

mucus, and would therefore entitle them to be placed among the mucous secretions.⁸ This opinion appears to be confirmed by the nature of the organ which produces them, which is not a membranous surface, but is a part possessed of a proper glandular structure.

A considerable part of the seminal fluid appears to be mucus, although both from its physiological and its physical properties it would seem to contain something of a peculiar or specific nature, upon which we may presume that its appropriate action more especially depends. What is commonly styled the pancreatic juice, is described to be a substance resembling saliva, and we are informed that the anatomical structure of the pancreas is very similar to the salivary glands of the fauces; but I believe that we have no very accurate information on either of these points.⁹

⁸ Journ de Phys. t. xxxix. p. 256. et seq. It would appear from Berzelius's analysis of the humours of the eye, that their chemical constitution might induce us to place them in this class, rather than in the albuminous, although they do not possess the physical properties of the mucous fluids. The "peculiar matter," which forms between 35 and 36 per cent. of the crystalline lens, so far as its properties are detailed, appears to differ from every other animal substance with which we are acquainted; Med. Chir. Tr. v. iii. p. 254; Ann. Phil. v. ii. p. 386. Fourcroy and Vauquelin also examined the nasal mucus, and the result of their examination was, that it very nearly resembles tears in all its chemical relations; they particularly state, that it contains uncombined soda, p. 359; but this does not agree with my own experience.

⁹ De Graaf's Treatise on the Pancreas, Tract. Anat. Med. &c. may be referred to, as the production of one of the most eminent anatomists of the 17th century, giving an account of all

If the idea be correct that the substance which gives the mucous secretions their characteristic properties be albumen in the coagulated state, it will follow, that the solid membranous bodies must belong to this class rather than to the albuminous, and ought indeed to be considered as the completion of that process, of which a mucous secretion is the first step. But it would be premature, in the present state of our knowledge, to proceed upon such strictly technical principles. I may farther remark, that if the two classes of albuminous and mucous substances be so nearly connected, as this view of the subject would suppose them to be, we may readily imagine that a slight change in the action of the secreting organ may effect a change in the substance produced, and convert a mucous to an albuminous secretion or the contrary; and I am disposed to think that I have witnessed several examples of this kind of conversion.¹

that had been discovered or imagined upon the subject. We may presume that the descriptions are correct, but the work is extremely diffuse, and contains a large portion of physiological and pathological hypothesis, which is now entirely superseded. See also Boerhaave, *Prælect.* § 101. cum notis; Haller, *Prim. Lin.* cap. 22; and *El. Phys.* lib. xxii; Sæmmering, *Corp. Hum. fab. t. vi.* p. 142. . 8; Blumenbach, *Inst. Phys.* § 24; Fordyce on Digestion, p. 70. . 2.

¹ A high degree of mercurial action on the salivary glands, appears to convert the fluid which they secrete into a substance of an albuminous nature, while a certain state of irritation in some of the serous membranes has caused them to secrete a mucous fluid; see *Med. Chir. Tr.* v. x. p. 80, 1; and v. xiii. p. 73. et seq. For remarks on the mucous secretions, the student may

The fourth class of secretions, the gelatinous, are so named from their essential characters depending upon the jelly which they contain, the specific property of which is to liquefy by heat and to become concrete by cold, exhibiting the phenomena to which the term gelatinization is applied. Jelly was formerly supposed to be one of the constituents of the blood, and to enter into the composition of several of the animal fluids, but more accurate experiments have proved that this opinion is erroneous.² It is, however, found very plentifully in many of the solids, and particularly in the membranous or white parts, although it is not confined to them.³ It may seem remarkable, that while jelly is so abundant in various animal solids, it should not exist in any of the fluids, notwithstanding its solubility in water. As it is not found in the blood, it follows that it is the result of a proper secretion; yet as we know of no organ especially appropriated to this office, it would seem to follow, either that it must be secreted by the minute

consult Thomson's Chem. v. iv. p. 424, 5; 525. et seq. Thenard, *Traité*, t. iii. p. 687. et seq. Henry's Elem. v. ii. p. 366, 7. Berzelius in Ann. Phil. v. ii. p. 381. et seq.; and Med. Chir. Tr. v. iii. p. 242. et seq. Magendie, *Physiol.* t. ii. p. 365. et seq. Children's Thenard, § 270. Bostock, in Med. Chir. Tr. v. iv. p. 75. et seq.

² Med. Chir. Tr. v. i. p. 71. 3; and v. iii. p. 233; Ann. Phil. v. ii. p. 205.

³ Berzelius, however, appears to think that jelly does not actually exist as one of the constituents of the body, but that it is generated by the boiling, instead of being merely extracted by this process; View of Animal Chemistry, p. 50.

capillaries that are dispersed over the parts where jelly is found, or that we are led to regard it as an extra-vascular production, being formed by some change in the elements of the body from which it is composed, after it leaves the arteries. We learn from an interesting experiment of Mr. Hatchett's, that albumen may be converted into jelly by digestion in diluted nitric acid;⁴ and there is reason to suppose that the conversion is produced by the addition of a portion of oxygen to the albumen, a conclusion which is in some degree confirmed by the ultimate analysis of the two substances.⁵ It is therefore reasonable to suppose that the same change may take place in the living body; and that the albumen which is conveyed by the capillary arteries, either while it remains in these vessels, or when it is discharged from them, is united to a portion of oxygen, and thus converted into jelly.

It is to be observed in respect to the relation between these two substances, that jelly exists in

* Phil. Trans. for 1800, p. 385. Fourcroy maintained an opinion that jelly is constituted by the combination of a portion of oxygen with albumen; Ann. Chim. t. iii. p. 261; but it does not appear that he ever actually effected the conversion, although this discovery has been ascribed to him; Thomson's Fourcroy, v. iii. p. 273.

⁵ According to MM. Guy-Lussac and Thenard, the following are the elements of albumen and jelly; Children's Thenard, p. 357; Thenard, Chim. t. iv. p. 401.

	Carbon	Oxygen	Hydrogen	Nitrogen
Albumen,	52.883	23.872	7.54	15.703
Jelly	47.881	27.207	7.914	16.988

much greater quantity in the corresponding parts of young than of old animals, so that those parts which in the early stages of existence are almost entirely composed of jelly, as age advances, are found to consist principally of albumen. We are not aware of any peculiarity in the state of the body during infancy, or in any of its functions, which can explain the superabundance of jelly at this period, nor do we know of any provision which there is in the constitution of the system for its more copious production at this period. We are equally ignorant of the mode in which it is afterwards disposed of: if it be taken up by the absorbents, it must be altered in its composition before it arrives at the blood, because we do not find any traces of it in this fluid, so that, perhaps, upon the whole, it is more probable that it undergoes some farther change, as we are not acquainted with any purpose which it serves while in the state of jelly, nor is it easy to understand in what way it is removed from the system. Jelly is never found but in connexion with a membranous substance, between the fibres or interstices of which we may presume that it is deposited, but there are some cases in which the jelly seems to form by far the greater proportion of the compound.

One of the parts of the body from which jelly is procured the most copiously is the skin, but it is somewhat doubtful in what state of combination it exists there; whether it be dispersed through a substratum of coagulated albumen, which appears generally to form the basis of the animal solids, or

whether the jelly be itself organized, a supposition which is rendered probable by the large proportion which it forms of certain substances. In isinglass, for example, the insoluble part is not more than about 1·5 per cent. of the whole, and unless we conceive the jelly, in this case, to form a mere concretion (an idea which is inconsistent with all our conceptions of the constitution of the animal body), we are almost reduced to the necessity of supposing the jelly itself to be organized. Like the albumen, jelly is nearly free from salts or any other extraneous substances.

The fifth class of secretions, the fibrinous, are so named from their resemblance to the fibrin of the blood, and from this being the probable source whence they are immediately derived. They differ from those that have been hitherto examined in the circumstance of their containing a larger proportion of nitrogen, or being, as it is said, more completely animalized, in their chemical composition,⁵ while in their physical structure, they retain the peculiar fibrous texture of the substance from which they are produced. In this class we must place the muscular fibre under all its various forms, which, whether con-

⁵ MM. Guy-Lussac and Thenard's analysis of fibrin is as follows:

Carbon 53·360, Oxygen 19·685, Hydrogen 7·021, Nitrogen 19·934; thus giving us 2·946 per cent. more nitrogen than in jelly, and 4·229 per cent. than albumen; *Researches*, t. ii. p. 350; Thenard, *Traité*, t. iii. p. 523, and t. iv. p. 305; *Children's* Thenard, p. 357. It is generally admitted that the elementary constitution of the pure muscular fibre is identical with that of the fibrin of the blood.

stituting the long fibres of the proper muscles, or the short ones of the muscular coats, appears to possess exactly the same chemical composition, and nearly the same physical form and arrangement with the fibrin of the blood.⁶ This is one of those cases where the effect of secretion appears to consist merely in separation, and this we may conceive to be accomplished by the separated substance having been simply discharged by the mouths of the capillary arteries, and deposited in its appropriate situation in the body, so as to be adapted, without any farther change, to the office which it is afterwards to serve in the animal œconomy.

It may be questioned whether there be any substance except the muscular fibre which ought to be arranged under this division. The other constituents of the body which exhibit a fibrous structure are, for the most part, what have been already included among the albuminous secretions, as being formed of this substance in the coagulated state, so that, upon the principles of the chemical arrangement, it appears necessary to include them in this division. It must, at the same time, be admitted, that the difference between the elementary constitution of albumen, jelly, and fibrin, is not very considerable, nor is it

⁶ Cuvier indeed observes, *Leçons*, t. i. p. 90, 1. that fibrin is not found in any of the food that is taken into the stomach, and concludes that it is formed by respiration; the operation of this function he supposes is to remove carbon and hydrogen from the blood, and consequently to leave in it a larger proportion of nitrogen.

very decisively established, yet there appears reason to conclude that an essential difference between them does exist. There are some other parts of the body which also possess a fibrous texture, such as the basis of the cutis, but this would seem to have more relation in its chemical composition to albumen or to jelly, than to fibrin. The fibrous coat of the arteries belongs to the class of substances which we are now examining, as also the fibres of the iris,⁷ and probably both of these must be considered as nothing more than mere varieties of the muscular structure. Fourcroy and Vauquelin have described a peculiar substance of a fibrous texture, which is found in the seminal fluid, and would appear to compose the specific part of this secretion, which, perhaps, ought to be referred to this class.⁸ A fibrous substance was

⁷ I have remarked above, v. i. p. 399, that Prof. Berzelius and Dr. Young conceive the chemical composition of these parts to differ from that of the proper muscular fibre; with every feeling of respect with such high authority, it appears to me that the experiments are not sufficiently decisive to enable us to form an opinion upon the subject. It would be desirable to compare the elementary analysis of the muscles of fishes and of the mollusca with those of the mammalia, in order to ascertain with what variety of chemical composition muscular contractility can be connected; we should probably find some difficulty in reconciling the chemical with the physiological arrangement.

⁸ Ann. Chim. t. ix. p. 64. et seq.; Thomson's Chem. t. iv. p. 534. 7; Thenard, Traité, t. iii. p. 694, 5. The nature and functions of the spermatic animalcules, which formerly gave rise to so much controversy, Blumenbach, Inst. Phys. § 528. and note (G), and the existence of which appears to be confirmed by the late observations of MM. Prevost and Dumas, Edin.

found by Dr. Marcet composing the basis of a urinary calculus, and similar substances have been occasionally met with in other morbid concretions, which are lodged in the different cavities or passages of the body; but it is probable that these were merely portions of fibrin that had been effused from a ruptured vessel, and not the result of any new action of the vessels.⁹

We now arrive at a class of bodies that are more distinct from any of those that are found naturally existing in the blood, and which we may therefore suppose to be the result of a more elaborate or complicated action of the secretory organs, the oleaginous secretions, those that derive their essential character from the presence of an oily ingredient. These compose a numerous, and at the same time, a considerably varied class of substances, in some of which the oil *Phil. Journ.* v. vii. p. 247, will be more properly considered hereafter.

⁹ Perhaps the synovia ought to be included among the fibrinous secretions, as we are informed by Margueron, that its specific character depends upon a substance of a fibrous texture, but the experiments do not enable us to decide, whether it be of the nature of muscular fibre or of membrane; *Ann. Chim.* t. xiv. p. 123; Thomson's *Chem.* v. iv. p. 532. . 4; Thenard, *Traité*, t. iii. p. 685, 6; Henry's *Elem.* v. ii. p. 433, 4; See Vauquelin's *Analysis of Synovia from an elephant*; *Ann. Chim. et Phys.* t. vi. p. 399. et seq.; and *Journ. Pharm.* t. iii. p. 289. et seq.

I had once an opportunity of examining a fluid from the cavity of the knee joint; it consisted of water holding in solution about 5 per cent of albumen, in which a number of flakes or masses were floating, that appeared to be composed of coagulated albumen; *Med. Chir. Tr.* v. iv. p. 74.

is nearly in a state of purity, or at least forms the greatest part of the body in question, while in the others, the oil is mixed with other animal principles, in such a manner, that it is not easy to decide in which division the substance under consideration ought to be placed. As a matter of convenience I have thought it better to place every substance in this class, that contains oil in any notable proportion, or of which any of the specific characters depend upon oil, although the actual quantity of it may be less than that of some of the other ingredients. Of the oleaginous secretions, the first that claims our attention, both from its quantity and the state in which it exists, is the fat of all kinds, which is found connected with the muscles and many of the viscera. In its chemical constitution fat appears to agree very nearly with the expressed vegetable oils; like those it varies in its consistence, or rather in its freezing point, so as, in the ordinary temperature of the atmosphere, to be found sometimes in a solid state, as is the case with suet and tallow, and at other times perfectly fluid, as we find it more particularly diffused through the cellular texture of the cetacea. We are not acquainted with any apparatus that is appropriated to the secretion of oil, nor are there any facts which can enable us to decide positively upon the mode of its formation. As a substance of an oily nature has been said to enter into the composition of the chyle, and as the formation and deposition of fat appear to bear a relation to the quantity of chyle that is produced, it has been conjectured that

the oleaginous secretions originate in the process of chylification, but it may be objected to this idea that the fat cannot be detected in the blood. Individual cases are indeed recorded, where the blood has exhibited an appearance as if something like cream was floating in it, but we are not well informed of the nature of this creamy matter, it is only a rare occurrence, and should probably be considered as depending upon some morbid, or at least some unusual state of the system.¹

¹ Haller, *El. Phys.* lib. i. sect. 4. p. 35..9, conceived that the fat existed in the arterial blood, and exuded from it through small pores, with which the vessels were supposed to be furnished; Wm. Hunter, *Med. Obs. and Inq.* v. ii. p. 33..36, thought that the fat must be secreted by a specific organ, but the considerations which they have adduced in favour of their respective opinions are altogether of a general nature. Magendie, *El. Phys.* t. ii. p. 347, 8. places fat among the cellular exhalations, because no specific structure can be detected for its formation; but it appears inconsistent to call a substance an exhalation, which is so unlike any of the proximate principles of the blood. Sir Everard Home has adopted an original hypothesis respecting the physiological relations of fat; *Lect. on Comp. Anat.* v. i. p. 468. et seq. and *Phil. Trans.* for 1821, p. 34; he thinks that it is not a secretion, but that it "is formed in the colon, and is thence taken up into the blood vessels, and distributed to the different parts of the body." We should be disposed to receive this opinion with the deference which is due to such high authority, but it must be acknowledged, that the arguments by which it is supported are not the most direct. It is said to be "sufficiently proved by the mode in which adipocere is made," a process which, as far as I can perceive, bears but a very remote analogy to the functions of the colon. The experiments that are related, I must confess, appear to me to

But from the circumstance of there being no appropriate organ for the secretion of fat, and also from the fact of its being deposited in so many parts of the body, or rather connected with textures of such various descriptions, we may conclude that fat must be formed by some peculiar action of the capillary system generally. The effect may be produced either by some change in the action of the vessels themselves, or in the composition of the fluid which is

be very inconclusive. As an argument in favour of his opinion, Sir Ev. Home observes that Mr. Hatchett made "the very interesting discovery . . . that fat, spermaceti, and adipocere, are in reality the same substance;" Lect. p. 477. Upon referring, however, to the account of Mr. Hatchett himself, we find this philosopher, who is so distinguished for his accuracy, thus expresses himself; "The chemical distinction between animal fat, spermaceti, and adipocere, as far as relates to the effects of heated alcohol upon these substances, has been hitherto erroneously stated;" p. 481. I think it will be admitted, that when we reflect upon what is essentially involved in the hypothesis, that the fat is to pass unchanged through the lacteals, mesenteric glands, thoracic duct, lungs, and arteries, and is finally to be deposited by their capillary extremities, it must require very direct evidence of the formation of the fat in the colon, while we might presume that it would not be difficult to detect it in some parts of its progress. Sir Ev. Home very candidly relates an unsuccessful experiment which he performed for this purpose; Phil. Trans. for 1821, p. 34. For an account of the different adipose secretions, the student may examine Thomson's Chem. v. iv. p. 438. O; Thenard, Chim. t. iii. p. 620. 637; Henry's Elem. v. ii. p. 382, 3; and Blumenbach, Inst. Phys. sect. 33. I shall beg to refer to some experiments which I performed in the year 1807, on the action of alcohol, upon different oleaginous or fatty substances; Nicholson's Journ. v. xvi. p. 165, 6.

transmitted through them; and it is most probable that this last is the case, because the deposition of fat is obviously connected with the state of the digestive organs. We have no certain grounds for enabling us to judge from what part of the blood the fat is immediately produced, but perhaps it may be considered more probable that it is from the albumen than from the fibrin, as we should scarcely expect that after the fibrin has been elaborated in the blood, it should be again decomposed. It may, however, be remarked, on the other hand, that the fibrin appears to be the most variable in its proportions of any of the constituents of the blood, and that the formation of fat may be the means by which it is carried off, when it is formed in greater quantity than is required for the wants of the system.

If we consider fat in its chemical relation to the other constituents of the blood, either the albumen or the fibrin, we shall find that it differs from them in containing no nitrogen, little or no oxygen, and a less proportion of carbon, so that when they are converted into adipose matter, the nitrogen and oxygen are retained with a portion of the carbon, while the hydrogen and a portion of the carbon are separated and compose the fat. There are two modes in which we may conceive of this operation being performed, according as we suppose the fat to be produced while in the vessels themselves, or not until it is just upon the point of being excreted from them. In the first case the operation must consist in the abstraction from the fluid of its nitrogen and oxygen and a part

of its carbon; in the other, of the hydrogen and a part of the carbon, while the remainder of the elements are left in the vessels. The circumstances which favour the deposition of fat, are an excess of nutritive matter received into the blood, at the same time that the secretions or excretions of various kinds are not duly discharged; we may therefore suppose that in this case there is a provision in the system for the removal of the superfluous matter, and that this is done by various means and by various organs. The principal part of the hydrogen appears to be disposed of in the formation of fat, the carbon is carried off partly by this means, and partly by the lungs, while the nitrogen is, perhaps, principally removed by the kidney.

The secretion of fat, however it is effected, is an operation which proceeds with great rapidity, for we find that no animal solid is so quickly generated where circumstances are favourable for its production. It is, on the other hand, the substance which is first removed from the body, when the system is suffering from inanition or disease,² so as to indicate that the fat is the part upon which the absorbents are the most disposed to act, while it is, at the same time, the most readily deposited by the capillary arteries. The great facility with which fat is generated, and afterwards removed from the system, has led some physiologists to regard it as one of those substances which are properly excrementitious, separated from

² Haller, *El. Phys.* xix. 2, 3.

the blood, rather in consequence of their being noxious, or at least useless, than from any important purpose which they serve. There may be some foundation for this opinion, but still, as I have had occasion to remark, it appears to be more analogous to the contrivance which we observe in the animal frame, and in the adaptation of its parts and actions to each other, to conceive that it may serve some important secondary purpose, and that even in the very act of being discharged it may produce some useful effect, which could not have been so well accomplished by any other means.

The uses which were assigned to the fat by the older physiologists, were principally of a mechanical nature, as that of lubricating the muscles and tendons, or giving them their proper degree of flexibility and suppleness; but it seems not easy to conceive, how the fat, lodged as it is in distinct receptacles, can produce this effect. In the cetacea, it may serve to render the body less disposed to part with its heat, and thus enable it to resist the cold medium to which it is exposed; but this purpose cannot be served by the fatty matter of quadrupeds, a large part of which is situated about the internal viscera.³ We seem, therefore, to be under the necessity of looking out for some other more important purpose which the fat may serve in the animal œconomy; and it has accordingly been suggested, that the adipose secretions may compose a reservoir of inflammable

³ M. Magendie regards the uses of fat to be principally connected with its physical properties; *Physiol. t. ii. p. 349.*

matter which will serve for the formation of carbonic acid in the lungs, when from any cause there is a deficiency of the usual supply as derived from the chyle. In ordinary cases, the thoracic duct pours into the venous trunks a quantity of chyle sufficient both for the growth and the nutrition of the body, and for the consumption of carbon by the lungs, but if, from any cause, the supply is insufficient, the absorbent system takes up the adipose matter from its various receptacles, and introduces it into the sanguiferous system, where it serves for the generation of carbonic acid, and consequent production of animal heat.⁴

Besides the fat under its ordinary forms, and in its various states of solidity, the marrow belongs to this class of secretions,⁵ and also the substances which are

⁴ See the elegant inaugural dissertation of Dr. Skey, "*Dé Materia Combustibili Sanguinis*;" also Prof. De la Rive, "*De Calore Animalis*," *passim*. That the fat is the origin of the inflammable matter which serves to maintain the animal heat, was maintained by Moschati, but his opinion was obscured by much false reasoning and incorrect experiment; see *Journ. Phys.* t. xi. p. 389.

⁵ Marrow has lately been analyzed by Berzelius, and was found to consist of the following substances:

Pure adipose matter	96
Skins and blood vessels	1
Albumen	} 3
Jelly	
Extract	
Peculiar matter	
Water	

produced from the sebaceous glands that are found in various parts of the body. These probably exist in a greater or less quantity in all animals, and impart to them their specific odours, many of which are very peculiar and powerful, and are connected with some of the most important instincts of the brute creation. Among the oleaginous secretions we ought probably to place the cholesterine which forms the basis of biliary calculi, although it differs from fat in some of its chemical relations, and it may moreover be doubted whether it be not formed after the substance of which it is composed leaves the vessels, and is simply lodged in the biliary ducts.⁶

We have several other secretions which owe some of their peculiar characters to the oil which they contain. Among these is milk, a very compound fluid, which is formed principally of oil in combination with albumen, so united as to form a kind of emulsion. By mere rest the greatest part of the oil separates, the albumen still remaining combined with the water and the other ingredients, from which it cannot be detached without the intervention of a chemical re-agent, which, by coagulating it, renders it easily separable by mechanical means. Milk likewise contains a saccharine matter, which assists in adapting it for its appropriate office, that of nourishing the young animal immediately after birth, and

⁶ See Chevreul, *Ann. de Chim.* t. xcv. p. 7. 10. and *Ann. de Chim. et Phys.* t. vi. p. 401. Spermaceti and wax are also adipose secretions, but they are not produced by the organs of the human subject.

may also have the farther use of contributing to preserve the milk in a fluid state, by rendering the emulsion of albumen and oil more perfect. Milk is secreted from a body which possesses all the appropriate parts of the glandular structure on a large scale, and appears to be possessed of a very elaborate organization. Were we to reason from the analogy of the other secretions, we might be led to form a conclusion, which is probably very different from the common opinion, that of the three substances which essentially compose milk, the sugar is the one for which the glandular apparatus is more particularly required. The albumen does not appear to differ essentially from the albuminous part of the serum, and we do not find that oil, in other parts of the system, requires any distinct gland for its formation.

It may, indeed, be thought an objection to this idea, that the kidney, in a certain morbid state, acquires the property of secreting sugar, from which it would seem that this substance, although so different in its nature from any of the constituents of the blood, may yet be formed from it, without any thing very peculiar or specific in the structure of the secreting organ, so that we might be inclined to ascribe the effect, rather to some alteration in the fluid that is brought to the part, than to the action of the organ itself. It is to be observed, however, that the sugar of diabetes exactly resembles vegetable sugar, while the sugar of milk differs from it in the proportion of its elements, and likewise in the result of the action of nitric acid upon it, which

produces mucic acid with the sugar of milk, and oxalic acid with the sugar of diabetes.⁷

7 The ultimate analysis of vegetable sugar, as given by the latest experiments, is as follows;

	Guy-Lussac and Thenard.	Berzelius, the average of 4 processes.	Prout.	Ure.
Hydrogen	6.9	6.882	6.66	6.29
Carbon	42.47	43.125	39.99	43.38
Oxygen	50.63	49.993	53.33	50.33
	100.00*	100.000†	99.98†	100.00§

The elements of the sugar of milk are as follows;

	Guy-Lussac and Thenard.	Berzelius.
Hydrogen	7.341	7.167
Carbon	38.825	39.474
Oxygen	53.834	53.359
	100.000	100.000**

It would appear from these analyses, that the sugar of milk contains more hydrogen and oxygen, and less carbon, than vegetable sugar. Dr. Prout, however, gives a somewhat different account of their comparative composition; he says, "sugar of milk yielded very near the same results" with vegetable sugar, and that their apparent difference is "to be attributed to the influence of the presence of minute portions of foreign matters, analogous, for example, to what occurs in the inorganic kingdom, in the mineral called arragonite;" *Med. Chir. Tr.* v. viii. p. 538. With respect to diabetic sugar, Dr. Prout could not find it to differ, in its ultimate analysis, from vegetable sugar; p. 537. Dr. Ure, on the contrary, finds its elements to be considerably different; he gives the following proportions;

Hydrogen	5.57
Carbon	39.52
Oxygen	54.91
	100.00††

As to the question of identity in this case, perhaps we ought

* *Research.* t. ii. p. 289.

† *Ann. Phil.* v. v. p. 264. 6.

‡ *Med. Chir. Tr.* v. viii. p. 536, 7. § *Phil. Trans.* for 1822, p. 467.

|| *Research.* t. ii. p. 293.

** *Ann. Phil.* v. v. p. 266.

†† *Phil. Trans.* for 1822, p. 467.

The milks of different kinds of animals have been minutely examined by various chemists, and although they have been found to differ considerably in the amount of their solid contents, and in the proportion which their constituents bear to each other, they essentially agree in their composition, as consisting of albumen, oil, and sugar, dissolved or suspended in a large quantity of water. In many cases we can perceive a relation between the nature of the milk and of the animal which is to be nourished by it, and we may remark generally that this fluid appears to be the combination, of all others, the best adapted for supplying the elements of nutrition in the early stages of existence, when there is a necessity for a copious supply of nutriment, while the digestive organs are in a state of extreme delicacy. Berzelius, who has lately analyzed cow's milk, has found it to contain 3 per cent. of the earthy phosphates, an evident provision for the formation of bone, while it would appear to differ from most of the other secretions, which consist principally of albumen, in containing no soda, either in the combined or uncombined state.⁸ The temporary existence of this secretion at a period when its utility is so obvious, with its cessa-

to depend more upon the effects produced by nitric acid, than upon the results of the elementary analysis. We are informed by Vogel, *Ann. Chim.* t. lxxxii. p. 156, that sugar of milk may be converted into a sugar resembling that from vegetables, by being digested with very dilute sulphuric or muriatic acid, thus furnishing an additional analogy between animal sugar and gum; See Thenard, *Chim.* t. iii. p. 549. . 1.

⁸ The following is Berzelius's analysis of skimmed cow's milk:

tion when no longer required, must be regarded as a very remarkable example of the adaptation of the system to the circumstances in which it is placed; we are quite ignorant of the means by which this change is brought about, or of the other changes in the system with which it is connected.

A substance which bears an analogy to milk, as

Water.....	928.75
Cheese, with a trace of butter.....	28.00
Sugar of milk.....	35.00
Muriate of potash.....	1.70
Phosphate of potash.....	0.25
Lactic acid, lactate of potash, and a trace of lactate of iron.....	6.00
Earthy phosphates.....	0.30
	<hr/> 1000.00

Ann. Phil. v. ii. p. 424.

From the above analysis, as well as from the remarks of Berzelius in *Med. Chir. Tr.* v. iii. p. 273. 6, we find that milk differs from blood, as well as from most of the animal fluids, in containing salts of potash, in place of the salts of soda; see also *Progress of Anim. Chem.* p. 111. 3. I may remark, that the absence of an uncombined alkali in milk would, on Berzelius's principle, exclude it from the class of secretions strictly so called; vide *supra*, p. 330. For the account of milk, beside the above references, see Parmentier and Deyeux, *Journ. de Phys.* t. xxxvii. p. 361. et seq.; Plenck, *Hydrol.* p. 86. et seq.; Fourcroy, *System*, v. ix. p. 468. et seq.; Haller, *El. Phys.* xxviii. 1. 16. 22; Henry, *Elem.* v. ii. p. 419. et seq.; Thomson's *Chem.* v. iv. p. 503. et seq.; Young, *De Lacte*, in *Sandif. Thes.* t. ii. p. 523.; this treatise, which was originally published in 1761, contains a very full account of all that was then known upon the subject, both with respect to human milk and that of other animals.

consisting of an intimate combination of albumen, and an oily ingredient, is the cerebral matter. It, however, differs from milk in the albumen appearing to be in a half coagulated state, as well as in its other constituents, the brain containing no saccharine matter, while a portion of the peculiar substance called osmazome is said to be found in it. Vauquelin, who performed an elaborate set of experiments on the cerebral matter, informs us that he procured a quantity of phosphorus from it; but from the nature of the process which he employed, it is not certain whether the phosphorus might not have been in the state of a phosphate. According to his analysis, rather more than one-fourth part of the solid contents consist of a fatty substance, and nearly one-third of albumen, the remainder being composed of osmazome, phosphorus, acids, salts, and sulphur.⁹

The seventh class of secretions, what I have denominated the resinous, are in their chemical properties considerably similar to the oleaginous, yet they appear to be so far distinguished from

⁹ Vauquelin's analysis is as follows:

Water	80
White fatty matter.....	4.53
Red ditto	0.70
Albumen	7.00
Osmazome	1.12
Phosphorus.....	1.5
Acids, salts, and sulphur.....	5.15
	<hr/> 100.00

Ann. Chim. t. lxxxi. p. 37; Ann. Phil. v. i. p. 332. et seq.

them, as to justify their being placed in a separate class. They derive their specific characters from an ingredient which is soluble in alcohol, and in various respects resembles a resin. Of these the most remarkable is the substance which constitutes the basis or specific ingredient of the bile. Bile is a very compound fluid, which is secreted by a gland of a peculiarly large size and elaborate structure. In consequence of its supposed use in the animal œconomy, and its remarkable chemical properties, bile has been frequently made the subject of examination; but although various chemists of the first skill and dexterity have exercised themselves in investigating its nature, we still find considerable discordance in their account of it; we may, however, conclude from them that it contains a substance which is analogous to a resin, upon which its peculiar characters most especially depend.¹ From the anatomical structure of the liver, and particularly from the connexion of its blood-vessels with the other parts of the sanguiferous system, it appears probable that the secretion of bile is more immediately made from venous blood.

¹ The chemists who have lately turned their attention to the analysis of bile, are Thenard and Berzelius; the former published an elaborate dissertation on the subject in 1805, in which he announced the existence of a peculiar proximate principle, which gives the bile many of its specific properties, and composes a large proportion of its solid contents; to this he gave the name of picromel, *Mem. d'Arcueil*, t. i. p. 23; also *Traité*, t. iii. p. 547, 8. He gives the following as the composition of ox bile;

The veins that collect the blood from the abdominal

Water	700
Picromel and resin.....	84.3
Yellow matter.....	4.5
Soda	4
Phosphate of soda	2
Muriate of do.....	3.2
Sulphate of do.....	0.8
Phosphate of lime	1.2
Oxide of iron	trace
	<hr/> 800.0

Mem. d'Arc. t. i. p. 38.

The constitution of human bile is considerably different, the picromel not being found in it, but its place partly supplied by what is termed resin.

Water	1000
Yellow insoluble matter.....	2 to 10
Albumen.....	42
Resin	41
Soda.....	5.6
Salts, the same as in ox bile....	4.5; p. 57.

Berzelius, however, calls in question the accuracy of Thenard's analysis; he does not admit of the existence of the resin as described by Thenard, but attributes the peculiar characters of bile to a substance which he simply denominates biliary matter; his analysis of bile is as follows;

Water	907.4
Biliary matter.....	80.0
Mucus of the gall bladder, dissolved in the bile	3.0
Alkalies and salts, common to all secreted fluids	9.6
	<hr/> 1000.0

Ann. Chim. t. 71. p. 220.; Ann. Phil. v. ii. p. 377..9; Med. Chir. Tr. v. iii. p. 241.

Dr. Thomson gives us rather a different statement of the result of Berzelius's analysis, which is quoted from his Swedish work;

viscera, which are more immediately concerned in

Water	908.4
Picromel	80.
Albumen	3.
Soda	4.1
Phosphate of lime	0.1
Common salt	3.4
Phosphate of soda with some lime ...	1.0
	<hr/>
	1000.0

Chemistry, v. iv. p. 522

Dr. Davy's analysis in *Monro's Elem.* v. i. p. 579, is as follows;

Water	86.0
Resin of bile	12.5
Albumen	1.5
	<hr/>
	100.0

For a farther account of the opinions that have been entertained respecting the chemical constitution of bile, the reader may consult Baglivi, *Diss.* 3. circa Bilem; Haller, *El. Phys.* xxiii. 3. 2. . 20; Fourcroy, *System*, v. x. p. 17. et seq.; Thomson, *Chem.* v. iv. p. 518. et seq.; Thenard, *Chim.* t. iii. p. 698. et seq.; Henry, *Elem.* v. ii. p. 412. et seq.; Plenck, *Hydrol.* p. 110. et seq.; Blumenbach, *Inst. Physiol.* sect. 25. For an account of the liver and its secretion see Sæmmering, *Corp. Hum. Fab.* t. vi. § 84. p. 163.

Dr. Thomson has analyzed picromel by means of the peroxide of copper, and found it to consist of the following ingredients;

Carbon	54.53
Oxygen	43.65
Hydrogen	1.82
	<hr/>
	100.00

The substance upon which he operated was procured by precipitating bile with sulphuric acid; the acid was separated by carbonate of barytes, and the fluid evaporated. What he obtained would therefore be "the biliary matter" of Berzelius; *Ann. Phil.* v. xiv. p. 70.

the process of digestion,² unite together into a large trunk, which is named the vena portæ; this enters

² Jacobson, of Copenhagen, has made some interesting observations on the comparative anatomy of the venous system of birds, reptiles, and fishes, which tend to illustrate this point; *Edin. Med. Journ.* v. xix. p. 78. This distribution of the veins is well displayed in Bell's *Dissect.* pl. 4. fig. 1. The case which is related by Mr. Abernethy, *Phil. Trans.* for 1793, p. 59. et seq., where the vena portæ terminated in the vena cava; and one by Mr. Wilson, where an unusual termination of the vena portæ was likewise observed, *Monro*, (3^d.) *Elem.* v. i. p. 564. 5. only prove that the elements of bile may be obtained from arterial blood. See the remarks of Mr. C. Bell, *Anat.* v. iv. p. 121. 2. and of Dr. Fleming, *Phil. of Zool.* v. i. p. 327. Bichat regularly discusses this question; but as the arguments which he adduces in favour of the opinion, are very hypothetical, it was not difficult to find answers to them; *Anat. Gen.* art. 6. t. 1. p. 406. 8. Among the earlier anatomists, one of the most minute accounts that we have of the liver and its functions, is by Glisson; *Anatomia Hepatis*. He discusses at length the question concerning the excrementitious nature of the bile, and concludes in favour of the doctrine; c. 38. et alibi. Another point which he very minutely examines, is respecting the connexion between the gall bladder, and the other parts of the hepatic system; the mode in which the bile is conveyed into and out of this receptacle, was a problem that the older anatomists found it very difficult to solve; Glisson states in detail the hypotheses of Laurens, Fallopio, Bartholin, Riolan, and others, which he formally discusses; C. 17. 20. See on the subject *Sæmmering*, t. vi. § 101. p. 194. 5. Before Harvey's discovery of the course of the circulation, the peculiar distribution of the blood vessels of the liver led to the opinion that this organ was to be regarded as the part whence the venous system took its origin. *Sæmmering*, t. vi. § 84. p. 181. 3, and *Monro*, (3^d.) *Elem.* v. i. p. 563. et seq. offer various considerations in favour of the opinion, that the vessels connected with the vena portæ, secrete the bile. See also *Blumenbach*, *Inst. Physiol.* § 427.

the liver, and divides into numerous branches, that are distributed through all its substance. The minute ramifications of these vessels terminate partly in the hepatic ducts, which contain the bile, formed, as it thus appears, from this venous blood, while the rest of it passes off by the hepatic veins, and is transmitted by the ordinary course of the circulation into the vena cava.³ Nothing certain is known respecting the mode in which venous blood is converted into bile, but there are certain facts and analogies, which would lead us to conceive, that it is more particularly derived from the red particles, and that the conversion is effected by the addition of oxygen to these bodies.

The great size of the liver, and the peculiar nature of the fluid which it secretes, has naturally led to various conjectures, respecting its relation to the general actions of the system, many of which are palpably incorrect, and founded upon fundamentally erroneous doctrines. It was a favourite opinion of some of the older physiologists, that the bile was a highly putrescent fluid, and they supposed that one principal use of the liver was to carry off from the system all the matter which was disposed to the putrid fermentation. By the modern physiologists, the bile has been supposed to be immediately useful in promoting the process of digestion;⁴ and it is very

³ Bell's Dissect. p. 22; the figures in plate 4 of this work, present an excellent view of the vascular system of these organs.

⁴ The purposes that the bile was supposed by the earlier physiologists to serve in the animal œconomy, are very numer-

probable that this is the case, but there are various pathological considerations which induce us to regard the bile as essentially an excrementitious substance, although, in conformity with the usual operations of the animal œconomy, some other important purpose may be served by it. When the venous blood becomes loaded with inflammable matter which cannot be discharged from the lungs, principally in consequence of the high temperature to which the animal is exposed, and when, from certain causes, one of which appears to be the increase of cutaneous perspiration, this excess of inflammable matter is not employed in the deposition of fat, the liver would appear to be the organ by which it is removed. In ordinary cases, the quantity discharged is small, probably no more than what is sufficient to preserve the liver in its healthy state, and to perform the secondary objects to which the function is subservient; but when, from a conjunction of circumstances, there is an excess of inflammable matter, its accumulation is prevented by an increased discharge of bile.

Another very important secretion, which may be classed under the head of the resinous bodies, is the urea, or that substance which constitutes the peculiar or specific ingredient in the urine. The urea does not indeed possess the characters of a resin in so remarkable a degree as the biliary matter, but it ap-

pears, but as they are, for the most part, quite hypothetical, it will not be necessary to examine them in detail; it may be sufficient to refer to Boerhaave, *Prælect.* § 99. *cum notis*, t. i. p. 213. et seq.; and to Haller, *El. Phys.* xxiii. §. 32., 35.

proaches more nearly to this class than to any of the rest. It ought probably to be regarded still more than bile, in the light of an excrementitious substance;⁵ for although it may serve other secondary purposes in the œconomy, there seems sufficient reason to believe, that its primary use is to discharge from the system a quantity of matter which is noxious, or at least superfluous. There is this peculiarity in the chemical nature of the urea, that it contains a very large proportion of nitrogen, so as to make it appear that the kidney is the outlet provided in the constitution, by which any excess of nitrogen is removed, or its accumulation prevented.

The quantity of nitrogen which is discharged in the form of urea is so considerable, even in animals whose food does not essentially contain this element, that we are led to inquire in the first place, how it is introduced into the system, and secondly, what purpose can be served by its introduction, when it appears to be discharged again almost as rapidly as it is received. While it was thought that the stomach

⁵ Berzelius supposes that there exists in various parts of the body a peculiar substance which is formed of decayed or decomposed animal matter, united to lactic acid or to some of the lactates; that this compound is taken up by the absorbents, carried to the blood, and afterwards discharged by the kidney; *View of Animal Chemistry*, p. 16, 82. We may suppose this substance either to be identical with, or to bear a near relation to the animal matter which is in the serosity of the blood. Blumenbach places the fluid of perspiration and the urine in the same class of excrementitious substances; *Inst. Phys.* sect. 34.

was the only channel through which nitrogen was introduced, there was great difficulty in accounting for the quantity of it, which was obtained by the graminivorous animals; but this difficulty is at least diminished, if not removed, upon the supposition that nitrogen may be absorbed by the lungs. And with respect to the second question, it may be sufficient, in this place, to reply, that from the great importance of the fibrin, as the source and origin of the muscles, and the seat of contractility, it was of the first importance to have a supply of nitrogen for its preparation, and that to ensure a sufficiency of it in every case of emergency, there must necessarily be, in most instances, an excess of it, which excess is carried off by the kidney.

The apparatus by which the urea is secreted, is of comparatively small size, but is complicated and elaborate in its structure, containing all the parts that are ever found in the composition of a gland. The kidney, like the liver, may be classed among the compensating organs, or among those which, independently of any useful office which they may habitually perform, have their actions occasionally increased for the purpose of supplying the deficiencies of other parts. Thus when a larger quantity of fluid is received into the stomach than can be imbibed by the absorbents, the residue is carried off by the kidney; and, in like manner, if the usual discharge from the lungs or the skin is prevented from taking place, we frequently observe that the kidney supplies the place

of these organs.⁷ Besides the urea, the urine contains several other substances, and particularly a great variety of salts, both earthy and neutral, which will belong to the next class of secretions.⁸

With respect to the relation which the urea bears to the other parts of the blood, before we can form any decisive conclusion on this point, it will be necessary to determine how far we are to admit of

⁷ Lining found that the quantity of the urine in summer was to that in winter, taking, in each case, the average of 30 days, as 1 to 2.03; *Phil. Trans.* for 1743, p. 509. The proportion of the urine to the perspiration in July, was as 977 to 1941; in January, as 1846 to 1006; *Ibid.* for 1745, p. 321. Stark found the quantity of urine in the day, to that in the night, during equal times, to be in the proportion of about 1.8 to 2.7; *Works*, p. 178; but his experiments were performed under such peculiar circumstances, that we can scarcely draw any general conclusions from them. He found the perspiration during the day to be rather greater in weight than the urine, while, during the night, its weight was not half that of the urine; *Ibid.*

⁸ Dr. Henry has announced the following list of substances, as having been "satisfactorily proved to exist in healthy urine;" *Elements*, v. ii. p. 435, 6.

Water.	Albumen.
Free phosphoric acid.	Lactate of ammonia.
Phosphate of lime.	Sulphate of potash.
Ditto of magnesia.	Ditto of soda.
Fluoric acid.	Fluate of lime.
Uric acid.	Muriate of soda.
Benzoic acid.	Phosphate of soda.
Lactic acid.	Ditto of ammonia.
Urea.	Sulphur.
Gelatin.	Silex.

According to Berzelius, the constitution of the urine is as follows; *Ann. Phil.* v. ii. p. 423.

the speculations of Berzelius, and of the conclusion which Prevost and Dumas deduce from their experiments.

Water	933.00
Urea	30.10
Sulphate of potash	3.71
Ditto of soda	3.16
Phosphate of soda	2.94
Muriate of soda	4.45
Phosphate of ammonia	1.65
Muriate of ammonia	1.50
Free lactic acid	} 17.14
Lactate of ammonia	
Animal matter soluble in alcohol	
Urea not separable from the preceding	
Earthy phosphates with a trace of fluete of lime ..	1.00
Lithic acid	1.00
Mucus of the bladder	0.32
Silex	0.03
	<hr/> 1000.00

In some minute points these two accounts do not coincide, but they sufficiently agree respecting the general nature of the urine, and especially in the great number of its ingredients, a circumstance which is a strong confirmation of its excrementitious nature, as a general outlet for whatever is not required in the system. It must be confessed, however, that there is a fact in comparative physiology, which is adverse to the excrementitious nature of urine; I allude to the observation of Dr. Davy, *Phil. Trans.* for 1818, p. 305, that the urine of serpents and lizards consists of nearly pure lithic acid. A substance was examined by Dr. Prout, which, according to the information that he received, was the only species of excrement that was discharged by the *boa constrictor*; but the remarks of Dr. Davy, in the paper referred to above, lead us to consider it probable, that it was, at least in a great measure, derived from

The object which they had in view was to ascertain the nature of the changes which are effected in

the kidney. Dr. Prout's analysis is as follows; 100 parts were found to consist of

Lithic acid	90.16	
Potash	3.45	
Ammonia	1.70	
Sulphate of potash, with a trace of muriate of soda.	.95	
Phosphate of lime	}80
Carbonate of ditto		
Magnesia		
Animal matter, consisting of mucus and a little colouring matter	2.94	

100.00

Ann. Phil. v. v. p. 415.

From some later observations by Dr. Davy, we have reason to suppose that the urine of toads and frogs differs from that of serpents and lizards, and more resembles the urine of the mammalia, in containing urea; Phil. Trans. for 1821, p. 98. The author adduces various considerations which lead to the conclusion, that the nature of the urine depends more upon the specific action of the kidney than upon any peculiarity in the diet; Phil. Trans. for 1810, p. 99. We have, however, the observations of Dr. Wollaston on the urine of birds, which leads us to the opposite conclusion; for he found the proportion of lithic acid in the excrements of different birds to vary from a $\frac{1}{100}$ part, in a goose which fed entirely upon grass, until, in birds which took animal food alone, it composed the greatest part of the whole mass. Besides the above substances, which Dr. Prout conceives to be essential to healthy urine, he enumerates the following, as occasionally found in certain morbid conditions of the system: "Nitric acid, various acids formed from the lithic, oxalic acid, benzoic acid, and carbonic acid, xanthic oxide, cystic oxide, Prussian blue? sugar, bile, and pus;" Inquiry into Diabetes, &c. p. 5.

the blood by secretion, and for this purpose they removed the kidneys from a living animal. The operation was not productive of any immediate injury to the functions, but in a few days various morbid symptoms arose, which seemed to indicate an inflammatory state of the system. The blood was carefully examined after death, and was found to contain a much greater quantity than ordinary of the animal matter which enters into the composition of the serosity, and by subjecting this substance to the action of various re-agents, and also by reducing it to its ultimate elements, it appeared that it exactly resembled urea, so as to lead the authors to conclude "that the urea of the blood is identical with that of the urine."⁹

⁹ Ann. Chim. et Phys. t. xxiii. p. 90. et seq. The following is the analysis of the urea procured from the blood of one of the dogs that had had the kidneys extirpated:

Nitrogen.....	42.23
Carbon.....	18.23
Hydrogen.....	9.89
Oxygen.....	29.65
	<hr/>
	100.00

This does not differ very much from Dr. Prout's analysis of urea:

Nitrogen.....	46.66
Carbon.....	19.99
Hydrogen.....	6.66
Oxygen.....	26.66
	<hr/>
	99.97

Med. Chir. Tr. v. viii. p. 535.

I may observe, with respect to the result of the experiments

The experiments of the Genevese physiologists, which seem to be countenanced by some lately performed at Tübingen,¹ would lead us to the opinion, that the serosity is the part of the blood from which the urea is more particularly derived, or rather that these substances are nearly identical, so as to give the kidney the mere office of separating the urea from the mass of blood, in which it existed ready formed.² I conceive that the facts of which we are in possession, however valuable and interesting, will scarcely entitle us to go the full length of this conclusion, but still I think it highly probable that an intimate relation subsists between the serosity of the blood and the urea. We are indebted to Dr. Prout for some very curious observations respecting the connexion between the urine and the digestive organs, which show in how great a degree the chemical properties of the former depend upon the condition of the latter; but these will be considered with more propriety in the next chapter.

The cerumen, or ear-wax, as it is termed, would of MM. Prevost and Dumas, that Haller maintains the possibility of urine being produced after the destruction of the kidney; *El. Phys.* vii. 1. . 9.

¹ *Edin. Med. Journ.* v. xii. p. 473. et seq.

² M. Berard remarks that the secretion of urine appears to have for its object the separation of the excess of azote from the blood, as respiration separates from it the excess of carbon; *Ann. Chim. et Phys.* t. v. p. 296. It is an ingenious observation of Berzelius, to which Dr. Prout is disposed to consent, that one great purpose which is served by the kidney, is the acidification of the constituents of the blood; *Inquiry*, p. 31.

appear, from the analysis of Vauquelin, to have a relation to the resinous secretions;³ and there are some substances derived from different species of animals, as civet, musk, and castor, which may be placed in the same class.⁴ We must also refer to this class the peculiar substance which was first pointed out as a distinct animal principle by Rouelle, and was afterwards more accurately examined by Thenard, and named by him osmazome.⁵ It was originally procured from the muscular fibre, of which it forms one of the component parts, and appears to be that ingredient upon which the peculiar flavour and odour of the flesh of animals principally depends.⁶ According to some experiments, of which we have the detail in three dissertations lately published at Tubingen, by Gsell, Gmelin, and Wienholt,⁷

³ Fourcroy, *System*, by Nicholson, v. ix. p. 451, et seq.; Thomson, *Chem.* v. iv. p. 523.

We have a valuable paper by Dr. Haygarth on this subject, the principal object of which is to point out the best means of dislodging it from the ear when it has accumulated there in an excessive quantity; we should be induced from his experiments to conclude that a considerable proportion of it consists of a mucous substance, depending, however, for its specific properties upon a body resembling the resin of the bile; *Med. Obs. and Inq.* v. iv. p. 198. et seq.

⁴ Thomson, *Chem.* v. iv. p. 441. et seq.; Thenard, *Chem.* t. iii. p. 777. et seq.

⁵ Thomson, *Chem.* v. iv. p. 425; Henry, *Elem.* v. ii. p. 465.

⁶ Berzelius, however, doubts whether osmazome is to be regarded as a distinct proximate principle; he seems to consider it as a compound of animal matter with lactate of soda; *Ann. Phil.* v. iii. p. 201. note.

⁷ *Ed. Med. Journ.* v. xii. p. 473. et seq.

osmazome is found in most of the component parts of the body, as well solids as fluids, although in very different proportions. It was found by Gsell to be much more copious in the muscles than in the bones and tendons, and in the muscles of old, than in those of young animals. Gmelin's experiments were particularly directed to the composition of the kidney; he examined this organ in the human subject, in the ox, and in the rat, and the result was, that a considerable proportion of osmazome was, in all cases, detected in the different parts of it, along with various neutral and earthy salts.

Weinholt extended his examination to other parts of the body, and found them all to contain certain portions of this substance. Some of the most important of his results are on the comparative quantity of osmazome, procured from blood as taken from different vessels; the greatest quantity was obtained from the vena portæ, a smaller quantity from the vena cava, and least of all from the aorta. He seems to consider the animal matter in the serosity as a compound of osmazome and urea, an opinion which nearly coincides with that maintained by Prevost and Dumas.⁸ This view of the subject, to a certain extent, countenances the idea of Berzelius, that the serosity consists of decomposed matter, which is carried into the blood-vessels, in order to be afterwards removed from the system, while it tends to throw some doubt upon the correctness of the

⁸ Ann. de Chim. et Phys. t. xxiii. p. 94.

deductions made by Prevost and Dumas from their experiments, so far as respects the theory of secretion generally.⁹

The eighth and last class of secretions are the saline, a very numerous set of bodies, which are found dispersed over every part of the system, and more or less mixed with almost all its constituents. They consist of acids, alkalies, and neutral and earthy salts.¹

⁹ The observations of Jacobson referred to above, p. 368, on the comparative anatomy of the venous system of the abdominal viscera, tend to show, that there is some connexion between the functions of the liver and the kidney, and might lead us to suppose that these organs are both of them rather excrementitious than secretory. That there is some connexion between the functions of the liver and the kidney seems to be proved by a singular circumstance stated by Mr. Rose; *Ann. Phil.* v. v. p. 424. 7; confirmed by Dr. Henry, *Ibid.* v. vi. p. 392, 3; that in hepatitis there is no urea secreted by the kidney. Should this be found to be a general occurrence, it would seem to indicate, that in some way or other, the secretion of the bile is a preliminary step to that of the urea, but we do not possess any data, either anatomical or pathological, which can enable us to determine how this preliminary change is effected.

The following acids are generally recognized as entering into the composition of animal substances, for the most part, however, in combination with an alkaline or earthy basis; the phosphoric, the muriatic, the sulphuric, the fluoric, the lithic, the lactic, the benzoic, the carbonic, and the oxalic, as existing in certain species of urinary calculi. To these we may add some others of more doubtful existence, such as the rosacic and the amniotic; there are also other acids which we obtain in our examination of animal substances, as the prussic, and the mucic, which appear to be generated during the process. Soda, potash, and ammonia, are all found in the animal fluids, the soda alone in the uncombined state. Of the earths, lime is by far the most

The most important of these, both from the quantity in which it exists, and its uses in the system, is the phosphate of lime, which constitutes the earthy matter of the bones, giving them their hardness and solidity, and composing a large part of their substance; but, with the exception of the bones, the fluids generally contain more saline matter than the solids. A certain quantity of salts is always present in the blood, and it would appear that the class of albuminous secretions contain nearly the same kind of salts, and in the same proportion, and this, as I remarked above, without any exact relation to the quantity of animal matter. The proportion of saline matter that is attached to the solid albuminous secretions, and to the gelatinous, is much smaller; the pure oleaginous secretions appear to be entirely destitute of any saline impregnation, while, on the contrary, the compound oleaginous secretions contain it in considerable quantity. It is found still more plentifully in the resinous secretions, and more especially connected with the urea, in the urine, where we meet with the greatest variety of salts, and where, with the exception of the bones, the saline substances exist in the greatest proportion of any part of the body.

It has been supposed that a reference to the nature of the salts, which are found in the animal body, is abundant; magnesia is found in small quantity, and also silex. Sulphur, phosphorus, iron, and, according to Vauquelin, Nicholson's Journ. v. xv. p. 145, manganese, appear also to exist in some of the constituents of the animal body, which, although not properly saline, may be conveniently placed in this class, in consequence of their relation to the salts.

and quantity of the saline substances that are found in the secretions, might enable us to form a natural classification of them, which would throw some light on the mode of their production, and even on the nature of secretion in general. Upon a principle of this kind, Berzelius divided all these bodies into secretions and excretions, the first being always essentially alkaline, and the latter acid;² but this, I conceive, would exclude many substances to which the title of secretion strictly belongs. It does not appear to me that we can lay down any arrangement of this description, which will apply to all the substances in question, but there may, perhaps, be a foundation for a division of them into four classes: 1. Those that are nearly without any mixture of salts; 2. Those which possess a definite quantity of salts, and this different from what exists in the blood; 3. Those containing salts, similar both in their nature and quantity to those in the blood; and 4. Such as contain salts different from those in the blood, and which are also variable in quantity. The fat, the saliva, the fluid discharged from the serous membranes, and the urea, may be taken as an example of each of these divisions. If we inquire in what way we are to connect the constitution of these substances with the supposed mode of their production, we may consider the two first, as the effect of proper secretion, where a substance that did not previously exist is elaborated by the action of the vessels; the third, of transudation,

² *View of Animal Chemistry*, p. 61, 2.

where, in consequence of an operation, that is, in a great measure, mechanical, a certain portion of the blood is, by a kind of filtration, strained off from the mass; while the fourth will constitute the excretions, substances which consist of the residual mass of the blood, after the secretions and transudations have been removed from it. If we apply this principle to the secretions as we have found them actually to exist, we must consider the solid albuminous, the gelatinous, and the simple oleaginous, as the only substances belonging to the first class; the mucous, the fibrinous, and the compound oleaginous to the second; the liquid albuminous will belong to the third; while the aqueous and the resinous will be placed in the fourth.

A very curious and important physiological question here presents itself respecting the origin of these salts, and more especially concerning the earth of bones, whether it is actually formed in the body, or whether it is, in the first instance, received into the system along with the aliment, and after being conveyed into the blood is separated from it, and gradually accumulated in the different organs, of which it afterwards forms a constituent part. The question becomes particularly interesting as it respects the physiology of some of the inferior orders of animals, and the connexion which they have with certain geological phenomena. A great proportion of the substance of several of the testacea and crustacea, consists of carbonate of lime, and it appears probable that many of the large calcareous strata which exist in

different parts of the world, have originated from the detritus or decomposition of these animals. We are then naturally led to inquire what is the origin of this lime? Did it exist in some other form previous to the creation of these animals, did they receive it into their system and organize its particles, so as to mould it into their shells and crusts, or have their digestive and secreting organs the power of actually generating lime? The former opinion is the one that appears the most obvious, and accords the best with our ideas of the usual operations of nature; but it is rendered improbable from the immense quantity of matter which the animals must have appropriated to themselves, and it is not very easy to conceive in what state the lime could have existed previous to its reception into their system.

The same kind of question occurs with respect to vegetables, and although they differ so essentially from animals as to render it dangerous to extend the analogy from the one to the other, yet in this particular point, they appear to be placed in precisely a similar situation. A considerable part of the solid matter of vegetables consists of carbon, and they also contain small quantities of various earths and metals, but as these substances are insoluble in water, which is the only medium through which plants are supposed to obtain their nourishment, it has been asked, how are they procured by the plants? Are they suspended in a state of minute division in the water which is absorbed by their vessels? can they be derived from the atmosphere? or do the plants actually

generate them? The question, as it respects both animals and vegetables, has been attempted to be resolved by direct experiment.

A set of experiments were performed on this subject by Vauquelin. They consisted in ascertaining the exact quantity of earthy matter which entered into the composition of the shell of the eggs, and also of the excrement of fowls; he carefully analyzed the food which they received, so as to learn precisely the quantity of earthy or saline matter which it could derive from this source, and then compared this with the quantity of lime and other earths which was found in the egg shells or the excrements.³ Another train of experiments was performed by Dr. Prout. He examined, with his usual accuracy, the composition of recent eggs, ascertained the nature and amount of their elements, and compared these with the composition of eggs after incubation, when the chick was fully developed. There are considerable difficulties attending experiments of this kind, and they require a degree of continued accuracy which few individuals are disposed to devote to them; but in the case of the two chemists above mentioned, no

³ He examined the relation which these quantities bore to each other in the male and female, and in the latter during the period of laying her eggs, and other analogous circumstances, and was led to draw the following conclusion, that a quantity of lime had been formed by the digestion, and the assimilation of the oats, that a portion of phosphoric acid had also been formed, that a certain quantity of carbonate of lime had been produced, and that a small quantity of silex had disappeared; *Ann. de Chim.* t. xxix. p. 3. et seq.

deficiency of this kind can be suspected, and in both cases the results appeared to indicate that the animal had acquired a greater quantity of earthy matter than could be accounted for, except by supposing that, in some way, a quantity of it had been developed by the vital powers.⁴

⁴ Prof. Berzelius, referring to Vauquelin's experiments, unhesitatingly concludes, that the earthy substances, which are evacuated by the animals, "must be capable of being composed or decomposed, as occasion requires, by the processes of organic chemistry;" View, p. 73, et seq. Dr. Prout is led to think that the earthy matter of the bones of the chick "does not pre-exist in the recent egg;" Phil. Trans. for 1822, p. 399. The average quantity of lime in the shells of eggs was found to vary so much, that it appeared impossible to determine positively whether the earth was derived from this source by chemical analysis; but he observes, there are "very strong reasons for believing that the earthy matter is not derived from the shell." He, however, adds with philosophic caution, "I by no means wish to be understood to assert, that the earth is *not* derived from the shell; because, in this case, the only alternative left me is to assert, that it is formed by transmutation from other matter; an assertion which I confess myself not bold enough to make in the present state of our knowledge, however strongly I may be inclined to believe that, within certain limits, this power is to be ranked among the capabilities of the vital energies." p. 400. The experiments of Fordyce, in which gold fish were kept in water that was supposed to be pure, and by being merely supplied with air, not only lived for many months, but increased very considerably in size, prove that the functions of these animals may be maintained in a perfect and healthy state for an indefinite length of time, merely by means of atmospheric air and water, but they do not exactly bear upon the question discussed in the text; Treatise on Digest. p. 78..0.

The experiments on vegetables were conducted upon similar principles: the composition of certain plants, seeds, or bulbs, was accurately ascertained; they were placed in distilled water, or planted in clean washed sand, sulphur, or some substance whence they could not be supposed to derive any extraneous matter, and were moistened with distilled water. After they had grown for some time they were carefully analyzed, and a comparison was made of the elements which they contained before and after their germination and growth. The results of these experiments, like the former, seemed to prove, that the solid matter which entered into the composition of the vegetables, in the more advanced periods of their growth must, in part at least, have been produced by some action of the vital powers, and could not have been obtained *ab extra*. For although it might be possible, which however does not appear to be the case, to refer the whole of the carbon to the decomposition of carbonic acid, as dissolved in the water employed, or diffused through the atmosphere to which they were exposed, and although a part of the earthy matter which was found in them, might be derived from the soil, and suspended in the fluid which entered into their vessels; it seems very difficult, if not impossible, to explain the whole of it upon this principle.*

* Dr. Thomson has given us a very good summary of the experiments that have been performed on this subject in his section on the "food of plants;" *Chem. v. iv. p. 320, et seq.* The conjoined evidence of the experiments of Braconnot, Shrader,

Of the nature of any powers which could produce such an effect as that described above, or even of their existence, independent of the case now under consideration, we are altogether ignorant, but should future experiments confirm the results of those that have been hitherto performed, and compel us to adopt this conclusion, it will be necessary to inquire what are the possible modes by which such extraordinary operations can be effected. In the present state of our knowledge it would be premature to enter into any long discussion upon the subject; I shall merely remark, that there appear to be only three possible or conceivable modes. Either some of the bodies which we suppose to be elements, as for example, oxygen, hydrogen, or carbon, are in reality compounds, and are decomposed by the powers of life, or these powers are capable of converting the elements into each other; or, in the third place, there is a creation of absolutely new matter. The first of these suppositions is in itself by far the most probable, and although it appears to be directly con-

and Einhof, seems to prove very clearly that we cannot account for the introduction of the constituents of vegetables from the soil or water with which they are in contact. Braconnot appears to have conducted his operations with great attention to accuracy. He concludes that "organic force, aided by solar light, developes in plants substances which have been regarded simple, such as earths, alkalies, metals, sulphur, phosphorus, carbon, perhaps azote;" *Ann. Chim.* t. lxi, p. 187, et seq. Saussure conceives that his experiments prove that plants, during their growth, acquire an additional quantity of carbon, but he supposes that they procure it from the air; *Recherches*, p. 50..3.

tradicted by numerous facts of the most decisive nature, yet, on the contrary, there are certain analogies which seem rather to favour it. But the further consideration of this question will more properly belong to the subject of digestion.

§ 3. *Of the Theory of Secretion.*

I must now proceed to offer some remarks upon the theory of secretion. The speculations that have been formed upon this subject are very numerous, but they may be all referred to five heads: that secretion is a species of fermentation; that it depends upon the immediate agency of what is termed the vital principle; that the secretions are separated from the blood by a mechanical process; that they are produced by a chemical action either of extraneous bodies upon the blood, or of the constituents of the blood upon each other; and lastly, it has been supposed to be effected by the agency of the nervous influence.

It will not be necessary to enter into any detailed account of the doctrine of fermentation considered as explaining the nature of secretion. It is generally supposed to have been originally advanced by Vanhelmont,⁶ and was adopted by Sylvius, Willis, and

⁶ I do not find in the writings of Vanhelmont any direct attempt to form a theory of secretion, but there are many passages in which it may be fairly ascribed to him by implication. Among others I may adduce the following: "Quomodo fermentum transmutationum parens sit, non melius, quam per Pyrotechniam inveni. Cognovi enim, quoties corpus dividitur in subtiliores atomos, quam suæ substantiæ ferat exigentia,

by the chemical physiologists generally of the sixteenth and seventeenth centuries.⁷ Each gland was supposed to possess a peculiar species of fermentation, which assimilated to its own nature the blood that passed through it, as in the case of the vinous and the acetous fermentations. But it may be sufficient to remark, that we have no evidence of the existence of these ferments in the different glands, nor have we any proof that the action of the glands, or the series of changes which they produce upon the fluids that pass through them, resembles any kind of fermentation with which we are acquainted. Perhaps, indeed, when we come to inquire a little more minutely into the nature of this hypothesis, as proposed by the chemical physiologists of the seventeenth, or even of the earlier part of the eighteenth century, we shall find that it may in fact be resolved into a mere chemical change of the blood into the secreted fluid, for they used the term fermentation in a much more extensive, or rather vague manner, than it is employed in the present day, applying it to any change which is effected in the elementary constitution of the substance operated upon, when it is produced without the visible interposition of external

continuo etiam sequi corporis illius transmutationem, excepto elemento. Quatenus haustum fermentum, arripiens præfatos atomos, eos alieno sui caractere imbut, in cujus susceptione fiunt divisiones partium, quas partium heterogeneitates, et divisiones, resolutio materiæ consequitur;" taken from the treatise, "*Imago fermenti imprægnat massam semine;*" *Ortus Med.* p. 93. § 23.

⁷ Haller, *El. Phys.* vii. 3. 28.

agents, or by the action of its constituents upon each other.⁸

Nor shall we find the hypothesis of the animists more satisfactory. It was zealously defended by the Stahlians of the seventeenth century, and has been received in our times, at least with certain modifications, by J. Hunter,⁹ Darwin, Blumenbach, Bichat and others,¹ and may perhaps be regarded as the prevailing doctrine of the London school. Yet, notwithstanding its general reception, I shall think it sufficient to refer to what I have already said respecting the vital principle and its supposed operations; we have no independent evidence of its existence, nor

⁸ It is in this way that the term fermentation is employed by Willis and Mayow; see the *Treatise de Fermentatione* by the former, and various passages in the essays of the latter. Cole, however, had a more correct and definite idea of the nature of fermentation; *De Secret.* c. 9. p. 32.

⁹ Mr. Abernethy's sixth lecture may be perused, as an excellent illustration of what has been already noticed, the error of supposing that we have obtained any real insight into a subject, merely by employing a new phraseology; see particularly p. 244, 5.

¹ Darwin's "Animal Appetency," *Zoonomia*, Sect. 37. § 2. v. 1. p. 463; Blumenbach's "Vita Propria," *Inst. Physiol.* Sect. 32. § 476. p. 252, and Bichat's "Vie Propre," *Anat. gen. systeme glanduleux*, art. 3. § 2. t. 2. p. 637, et seq. may be ranked among those mysterious doctrines, which it is sufficient to controvert by saying that these supposed agents have never been proved to exist, and are at the same time incomprehensible. Bichat, however, endeavours to show that the power of the nerves is not directly essential to secretion; *ubi supra*, p. 624. . 7; also *Anat. gen.* t. 4. p. 604. . 6.

The obvious objection to this hypothesis was the difficulty in conceiving how mere filtration could separate so many substances from one fluid; but in order to meet this objection two speculations were proposed, one of which it seems that we are indebted to Descartes, and the other to Leibnitz. The first of these philosophers proposed the whimsical and perfectly gratuitous supposition, that the particles of the various secreted substances were of different figures, and that the pores of the glands possessed the same figures, each gland therefore allowing those particles to pass through it which possessed the same figures with its own pores.³ The hypothesis of Leibnitz, although equally unsupported by facts, is less palpably absurd; he compared the glands to filters which had their pores saturated with their own peculiar substance, so as to admit of this substance alone passing through them to the exclusion of all the others, in the same manner as a paper saturated with oil prevents the passage of water, and vice versa.⁴

³ De homine, p. 18. § 11, et de form. fœtus, p. 212. § 25.

⁴ Quesnay adopts the hypothesis of filters, but adds to it the operation of the nervous influence; he supposes that secretion is accomplished by the joint action of "the sensibility of the filters" and the acrimony of the fluids; *Œcon. Anim.* t. 3. p. 37, 8; the section is entitled, "Affinity of the Secretory organs with the juice which filtre through them." Haller gives us the names of various eminent anatomists and physiologists who adopted the one or the other of these hypotheses. He is led to make the following observation, of the truth of which these pages afford us so many examples. "*Sæpius monui, felicibus exemplis expertus, raro eam esse hominum felici-*

As our knowledge of the nature of the secretions was gradually advanced, and when the improved spirit of philosophical inquiry taught us to reject such purely conjectural speculations, it was found necessary to examine a little more minutely into the circumstances which might be supposed to operate in the production of the secretion, and to compare these with the actual state and condition of the secretory organs. Still, however, the mechanical doctrines continued to prevail among the most enlightened physiologists, and both Boerhaave⁵ and Haller⁶ men-

tatem, ut vera sint, quæ facile et sponte quasi menti se offerunt." *El. Phys.* vii. 3. 29. Some judicious remarks on the mechanical hypothesis of secretion are contained in a paper of Winslow's; *Mem. Acad. Scien.* 1711, p. 241, et seq.

⁵ *Prælect.* § 253, cum notis.

⁶ Haller's doctrine respecting secretion is contained in the first 27 paragraphs of the 3d section of his 7th book; it may be asserted that no part of his great work displays in a higher degree his extensive information and correct judgment. It is to be observed that Haller laid it down as the foundation of his hypothesis, that the secretions all exist perfectly formed, or nearly so, in the blood, *El. Phys.* vii. 1. 8. and that he regarded the glands in no other light than as sieves or strainers to carry off their appropriate fluids, vii. 2. 1. et alibi. His great argument is the facility with which metastases take place, which, as he supposes, proves that the gland cannot form the substance which it discharges; he particularly notices the fact that urine is found in the fluids after the destruction of the kidney, vii. 1. 9. Although not directly applicable to the function of secretion, yet I am induced to refer my readers to a paper of Balguy's, on the mode of ascertaining the doses of certain medicines, *Ed. Med. Ess.* v. iv. p. 33, et seq., as a curious example of the length to which the mechanical physiologists carried their doctrines.

tained opinions respecting secretion, which are essentially of this description, although reduced into a much more rational and tangible form. Haller displays his usual candour and caution in forming his opinions upon the subject; he states the facts upon which he reasoned, endeavours to appreciate their value, and after maturely considering the premises, he deduced from them his conclusion. He proceeds upon the principle, that all the secretions are ready formed in the blood, but did not appear to think it necessary to inquire by what means they were originally generated there, or how they were introduced into it. Assuming, however, their existence, he conceives that there are seven causes which may contribute to their separation; 1. a difference in the nature of the blood itself; 2. the velocity of the blood as caused by the size of the vessel; 3. the transmission of the blood from one vessel to another which differs from it in size; 4. the angles at which the secreting arteries pass off from the trunk; 5. the course of the vessel, whether straight or winding; 6. the density of the vessel; and lastly, the structure of the excretory duct. There may be some foundation for all these causes; as affecting the contents of the vessels,

One of the rules to be observed is as follows; "You are to dose so much of the medicine as is spent on the stomach and intestines, directly as the constitution; and so much as is carried into the blood, as the square of the constitution, and the sum into the person's size is the quantity required;" p. 35. A less extravagant, but equally unfounded attempt is that of Gorter, in which he refers the whole affair of secretion to the physical properties of the fluid and the size of the vessels; *De Secrétione*; see particularly the accompanying plate.

although we shall probably conceive of them as very inadequate to produce the variety of substances that we meet with. In fact they may be all referred to the size of the vessels, and the velocity with which their contents are propelled through them; the formation of the substances, by the intervention of external agents, or the action of the constituents upon each other having been either not contemplated, or not conceived to form a necessary part of the hypothesis.

As animal chemistry was more attended to, and we became better acquainted with the changes which the component parts of the blood are capable of experiencing, by subjecting them to various chemical re-agents, it was conceived that all the secretions might be produced solely by the operations of chemical affinity. It does not very clearly appear with whom this theory of secretion originated. Perhaps Keill was the first who endeavoured to explain secretion upon chemical principles, but his opinions were altogether so imperfect, as to bear but little resemblance to the modern doctrine.⁷

The main argument for this hypothesis is, that by

⁷ Haller, *El. Phys.* vii. 3. 33. Keill, in his treatise on animal secretion, proposes to illustrate the following positions; "1. To show how the secretions are formed in the blood, before they come to the place appointed for secretion; 2. To demonstrate in what manner they are separated from the blood by the glands." p. 1. The 4th of the essays in the "*Testamina Medico-physica*" is nearly a translation of the above. The reasoning is strictly mathematical, and affords a very remarkable specimen of the misapplied learning of the mathematical physiologists. The doctrine of attraction, as applied to secretion, is particularly laid down in the 7th and 8th prop.

certain chemical processes, we are able to form from the blood, out of the body, substances similar to the secretions; hence, upon the principle of not unnecessarily multiplying causes, it is said that the same kind of operation must produce the secretions in the body. And this, it is supposed, may be effected by conveying the blood in its entire substance, or any of its components separately, as occasion requires, to different parts of the system, there to be subjected to the action of external agents; or by placing the entire blood, or any of its components, in such a situation, as that it may undergo the spontaneous changes to which it is liable. An argument has been urged in support of the chemical hypothesis, derived from our knowledge of the variety of substances which may be produced from only a very few elements, merely by their being united in different proportions. This is very remarkably the case with oxygen and nitrogen, which in one proportion compose atmospheric air, in another nitrous oxide, in a third nitric oxide, in a fourth nitrous acid, and in a fifth nitric acid, substances which differ from each other, at least as much as the secretions differ from each other, and from the blood. And what is still more in point, some of the late investigations into the atomic constitution of animal substances, exhibit the same production of new compounds by a different proportion of their elements. Dr. Prout has, with his usual sagacity, developed this kind of relation between the three substances, urea, lithic acid, and sugar, and shown how they may be converted into each

other by the addition or subtraction of single atoms of their constituents. Urea, which appears to be the substance secreted by the kidney in the healthy state of the functions, is composed of two prime equivalents of hydrogen, and one of carbon, oxygen, and nitrogen respectively; by removing one of the atoms of hydrogen, and the atom of nitrogen, we convert it into sugar, or by adding to it an additional atom of carbon into lithic acid.⁸

There are many instances of bodies, both of vegetable and animal origin, which are converted into each other by the operation of apparently slight causes, while we have likewise substances that possess specific and sufficiently distinctive characters, which yet seem to consist of the same elements, and almost exactly in the same proportion, so as to render it probable, that a very minute variation in the affinities might originally have produced one or the other of them, or after they were produced, might transform them into each other. As examples of the first species of operation, we may adduce the conversion of farina

⁸ Dr. Prout has constructed the following table for the purpose of illustrating his views on the subject; *Med. Chir. Tr.* v. viii. p. 540.

ELEMENTS.	UREA.		SUGAR.		LITHIC ACID.	
	Per Atom.	Per Cent.	Per Atom.	Per Cent.	Per Atom.	Per Cent.
Hydrogen . . .	2.5	6.66	1.25	6.66	1.25	2.85
Carbon	7.5	19.99	7.5	39.99	15.0	34.28
Oxygen	10	26.66	10	53.33	10	22.85
Azote	17.5	46.66			17.5	40.00
	37.5	100.00	18.75	100.00	46.75	100.00

into sugar, according to the process of Kirchoff, and of fibrine into adipocere, as in the well known case of the burial ground at Paris, both of which appear to be effected merely by the addition of a small portion of water, and as an illustration of the latter position, the elementary constitution of gum and farina, and of fibrin, albumen, and jelly, may be adduced. But in reference to the above facts, it may be said, if by so apparently slight a cause, as the addition of a small portion of the elements of water, an oily substance can be produced out of the body, there would seem to be no assignable cause, why the same change might not be effected in the vessels, by placing blood in a situation where it might either procure the elements that are necessary, or part with those that are superfluous. That changes of that description can take place in the fluids, while they are still circulating in the vessels, is proved by the action of the air upon the blood in the lungs, for whatever be our theory of the ultimate effect of respiration upon the system, we cannot doubt, that the air, by its chemical agency, is instrumental to the conversion of venous into arterial blood, while we have found it probable that the vessels of the lungs are capable of both absorbing and discharging certain substances through their coats.

It is further urged in support of the chemical theory, that the blood is a substance which we suppose to be peculiarly well adapted to experience changes of this description; it is composed of a number of ingredients, which are held together by a

weak affinity, liable to be disturbed by a variety, even of what might appear the slightest causes. And although we have been induced to conclude that mechanical causes alone are inadequate to produce the changes which take place in the blood, they may have considerable effect in modifying the operation of the chemical actions. For example, they may not only bring substances into contact or proximity, but they may render this contact more or less extensive, and may continue it for a longer or shorter space of time; they may subject the substances to various degrees of pressure, and may propel them through the vessels with various degrees of velocity, and in this way, they may not only determine the amount of effect to be produced, but they may vary the nature of it by protracting the action, or arresting it in different stages of its progress, and in this way infinitely vary the results. Now all these incidents must happen to the blood in the different parts of the circulation; it passes through vessels of various diameters, and with various degrees of velocity, and in short is subjected to all the actions which can arise from mere mechanical operations.

In order to assist us in conceiving how a variety of substances may be produced from a single compound, by the intervention of physical causes alone, we may suppose a quantity of the materials adapted for the vinous fermentation to be allowed to flow from a reservoir, through tubes of various diameters and with various degrees of velocity. If we were to draw off portions of this fluid in different parts of its course,

or from tubes which differed in their capacity, we should, in the first instance, obtain a portion of unfermented syrup; in the next we should have a fluid in a state of incipient fermentation; in a third, the complete vinous liquor; while in a fourth, we might have acetous acid. To apply these remarks to the blood; we know that mere rest allows it to separate into serum and crassamentum, and that if the crassamentum be formed under certain circumstances, the red particles are detached from the fibrin, which is left in a pure state. Then when we consider how many re-agents have the power of coagulating albumen, we shall not find it very difficult to conceive that this process may take place in the minute capillaries, and according to the degree in which the coagulation takes place, and consequently, in which the albumen is separated from the serum, so shall we have the serosity left, containing a certain quantity of albumen affording us the different kinds of fluids which compose the albuminous secretions. We farther find that by the action of dilute nitric acid upon fibrin and upon albumen, we are able to convert these substances respectively into adipose matter and jelly, changes which are probably effected by adding oxygen to the fibrin, and to the albumen; and there is some reason to believe, that by applying the same re-agent to the red particles we may produce a substance nearly resembling bile. Such facts as these seem to warrant us in concluding, that certain chemical operations, which it is not unreasonable to suppose may take place in the blood, can form from

it some of the substances, which in the ordinary course of the animal œconomy, are the result of secretion. That we are not able to imitate all the secretions, can scarcely be regarded as any decisive objection to the hypothesis, because it can only prove our ignorance of the intimate nature of some of the operations, without affecting our opinion respecting the means by which they are produced.

If we adopt the chemical theory of secretion, we must conceive of it as originating in the vital action of the vessels, which enables them to transmit the blood, or certain parts of it, to the various organs or structures of the body (the blood itself being previously prepared by the process of assimilation, and adapted for its appropriate functions, one of the most important of which is the formation of the secretions) where it is subjected to the action of those re-agents, which are necessary to the production of these changes. The re-agents themselves are, at least, in some cases, conveyed to the blood by muscular action, or by other vital operations, analogous to the contraction of the diaphragm, by which the air is received into the thorax. The remaining part of the process will be strictly chemical, or such as the substances would exercise upon each other, whenever they were placed in the same relative circumstances.

The last hypothesis of secretion which I proposed to notice, is that which ascribes it to the action of the nerves. It is supported partly by a number of facts and observations, which tend to establish a general connexion between secretion and the nervous

influence, and partly upon certain experiments that have been performed in order to show that the secretion of particular glands is deranged or destroyed by depriving them of their nerves. With respect to the first point, there are many well known occurrences, which clearly indicate an intimate connexion between the action of the nerves and the formation of the secretions. Certain secretions are encreased in quantity, and have their qualities materially altered by the intervention of mental emotions, and of various agents which we can only conceive to operate through the medium of the nerves.

So many examples of this kind will immediately suggest themselves to the mind, as to render it scarcely necessary to particularize them. The secretion of tears from the impressions of grief, and the encreased flow of saliva from the idea of grateful food, are among the most familiar instances of this nature. The process of digestion, as connected with the secretion of the gastric juice, is peculiarly liable to be affected by the state of the nervous system, so that, according to its excitement or depression, the fluid appears to be produced in greater or less quantity, the functions of the stomach being proportionably depraved, or even entirely suspended. The secretion of milk seems also to be very much under the influence of the nervous energy; not only does the mechanical excitement of the termination of the excretory duct encrease the action of the gland, by an operation which must have been conveyed to the

part through the nerves, but the increased flow of milk may be produced by causes which can only act upon the mind of the mother. There are many known occurrences of an opposite nature, which equally illustrate the subject under consideration, where the secretion of a gland is diminished or entirely stopped, in consequence of the removal of the exciting cause, which cause must have acted through the medium of the nerves. The case of the mamma may be again cited as a striking illustration of this position, for by the removal of the young from its mother, the secretion of milk is, after a short time, entirely suspended, in circumstances where the gland would otherwise have continued its action for an indefinite length of time.⁹

These facts, and many others of a similar description, prove a close connexion between the action of the glands and that of the nerves, but they do not absolutely prove any thing respecting the intimate nature of secretion; they only afford us a new illustration of what has so frequently fallen under our notice, that the different systems of which the body is composed are closely connected together. Nor is there any thing which ought to excite our surprise in these facts, nor do they appear, in any respect, adverse to the principles which we have laid down; for it cannot be thought more remarkable that a change in the effect should be induced by a change

⁹ Quart. Journ. v. i. p. 165, 6.

in the degree of the operating cause, than that the cause itself should originally be able to produce the effect.¹

An attempt was made by Sir Ev. Home, to establish a more intimate connexion between the action of the nerves and the secretory organs, by the intervention of the galvanic influence. The extraordinary power of decomposition which this agent had been discovered by Sir H. Davy to possess, taken in connexion with the electrical apparatus which is found in certain animals, suggested the idea that a similar kind of operation might be employed to act upon the blood, while the extreme sensibility of the nerves to the stimulus of galvanism, pointed them out as

¹ There is a fact that occurs with respect to birds, which ought probably to be referred to the influence of the nervous system over an organ of secretion, and which illustrates the extent of this influence in a remarkable manner, at the same time that it proves it to be exercised over a part that might have been supposed to be beyond the reach of its action. I refer to the formation of eggs in the ovarium. A bird which, when left in its natural state, would lay a few eggs only, if they be removed as they are produced, may be made to lay almost an indefinite number of them, and apparently without any derangement or injury to the system, or without the application of any external exciting cause. The only explanation that we can offer of this fact seems to be, that the instinct of the animal leads it to continue depositing its eggs in the nest until a certain number are accumulated; of the mode of operation in this case we are entirely ignorant, but we can scarcely doubt that it must be by the intervention of the nervous system. Berger, *De Nat. Hum.* p. 121. 3; and Bordeu, *Sur les Glandes*, § 98. et seq. may be consulted with respect to the influence which the nerves possess over the action of the secretory organs.

the parts that were the best adapted for effecting the change.² Dr. Wollaston, about the same time, formed a similar idea with respect to the agency of galvanism on the animal fluids, and illustrated his opinion by a very ingenious experiment, in which a low galvanic power produced the decomposition of a saline solution, and caused its component parts to transude through a portion of bladder, each of them being separately attracted to the corresponding wire in the interrupted circuit. Upon this experiment he remarks, that the quality of the secreted fluid may probably enable us to judge of the electrical state of the organ which produces it, as for example, "the general redundancy of acid in urine, though secreted from blood that is known to be alkaline, appears to indicate in the kidney a state of positive electricity; and since the proportion of alkali in bile seems to be greater than is contained in the blood of the same animal, it is not improbable that the secretory vessels of the liver may be comparatively negative."³ Some experiments were afterwards performed by Mr. Brodie, which seemed to indicate a close connexion between the secretion of the glands and the integrity of the nervous system,⁴ but scarcely any attempt was made to ascertain the

² Phil. Trans. for 1809, p. 385. et seq.

³ Phil. Mag. v. xxxiii. p. 488. O.

⁴ Phil. Trans. for 1811, p. 38, 9; and for 1814, p. 104. et seq. In the first of these papers Mr. Brodie found that the secretion of urine was suspended by the removal or destruction of the brain, although the circulation was maintained by the inflation of the lungs; in the second paper it is stated that when an animal is destroyed by arsenic, after the division of the par

extent of this connexion, or directly to prove its existence, until Dr. Philip entered upon the investigation.

He has endeavoured to identify secretion with the immediate result of the nervous influence by a series of very interesting and curious experiments on the effect that is produced upon the secretion of the gastric juice, by the division of the par vagum.

In the chapter on respiration I had occasion to refer to the effects which had been observed to follow from the division of these nerves, one of which was stated to be the derangement of the functions of the stomach. Blainville appears to have been one of the first experimentalists who minutely attended to this circumstance; it was subsequently noticed by Legallois,⁵ and Mr. Brodie,⁶ and Dr. Philip, on carefully repeating the experiment for the express purpose of noticing the effect on the stomach, announced, that after this operation, the digestive process was entirely suspended.⁷ But notwithstanding the apparently

vagum, all the usual symptoms are produced, except the peculiar secretion from the stomach.

Mr. Brodie's conclusion from his experiments was, that the nervous influence is a step in the process of secretion, but he does not decide that it is absolutely necessary to it. It appears that Berzelius had announced, some years before, his opinion of the connexion between the nerves and the function of secretion; See Phil. Trans. for 1809, p. 385; and that an hypothesis somewhat similar to Sir E. Home's had been advanced by Dr. Young in his lectures; Med. Lit. p. 110, 1.

⁵ Sur la Vie, p. 214, 5.

⁶ Phil. Trans. for 1814, p. 102. et seq.

⁷ Inquiry, p. 119..125.

simple and decisive nature of the experiment, the fact was controverted by physiologists of great respectability, who asserted, that after the division of the par vagum, the digestion was continued nearly in the natural state, or at most was only slightly retarded, so as to require rather a longer time for its completion. This opposition of opinion gave rise to a very warm controversy, in which experiments were adduced on both sides with equal confidence, and apparently with equally decisive results,⁸ when at

⁸ This controversy was carried on principally in the *Quarterly Journal*, v. vii. p. 161. et seq. and 349. et seq.; v. ix. p. 197, 8; v. x. p. 292. et seq.; v. xi. p. 45. et seq. and p. 320. et seq.; v. xii. p. 17. et seq. and p. 96, 7; it was conducted by Dr. Philip and Dr. Hastings on the one side, and by Mr. Brodie and Mr. Broughton on the other. The whole series of papers is well worth perusal, both as containing a number of curious facts, and still more as suggesting many important reflections on the mode in which philosophical discussions ought to be conducted. The experiments have been since repeated at Paris, by MM. Breschett, Edwards, jun. and Vavas seur; when Dr. Philip's fundamental positions were fully confirmed. See *Archives Generales de Medicine* for Aug. 1823, also Edwards de l'Influence &c. p. 527. We have, however, a still later series of experiments performed by MM. Breschet and Edwards, jun., in which they somewhat modify their former results. They suppose that the complete division of the par vagum, even with the precautions employed by Dr. Philip, although it very materially affected the process of chymification, does not entirely destroy it; they conceive that this diminished effect does not depend upon the absence of the gastric juice, but upon a paralysis being induced upon the muscular fibres of the stomach, in consequence of which the different parts of the alimentary mass are not duly brought into contact with the coats of the stomach,

length the very curious circumstance, to which I have already referred, was discovered, that the mere division of the nerves, and even the retraction of the divided ends, for the space of one-fourth of an inch, does not prevent the influence from being transmitted along them to the stomach, but that if a portion of the nerve be actually removed, or the ends folded back, the digestive process is suspended in the manner that was described by Dr. Philip.⁹ The original proposition of Dr. Philip is therefore established, that if the par vagum be divided in such a manner, as effectually to intercept the passage of the nervous influence, the digestion is suspended, and as this operation is supposed to be brought about by the action of the gastric juice, which is secreted from the surface of the stomach, it was concluded by him, that

so as to be exposed to the action of its secretions, that the effect of the galvanic influence is to restore the due action of the fibres; they inform us that a mechanical irritation applied to the lower end of the divided nerves produces a similar kind of change upon the food introduced into the stomach; from which they finally conclude, that the use of the par vagum, as connected with the functions of the stomach, is to bring the alimentary mass into sufficient contact with the gastric juice; Arch. Gen. de Med. The experiments are related with a sufficient degree of minuteness, and bear every mark of accuracy; but upon being repeated in London by Mr. Cutler, under the inspection of Dr. Philip and Mr. Brodie, so far as the main point was concerned, the effect of mechanical irritation of the lower part of the divided nerve, the results did not correspond with the results of MM. Breschet and Edwards, jun.; Med. Chir. Rev. v. iii. p. 589, O.

⁹ Phil. Trans. for 1822, p. 22, 3; Quart. Journ. v. xi. p. 325 .. 7, and v. xii. p. 19, O.

secretion is necessarily connected with the nervous influence, and cannot be performed without its intervention.

It was in connexion with the inquiry respecting the effect produced upon the stomach by the division of the par vagum, that Dr. Philip made his curious discovery concerning the power of galvanism in supplying the place of the nerves. The influence which the electric fluid, as excited by the galvanic apparatus, appeared to exercise over the muscles, as well as other circumstances, which seemed to point out a connexion between this agent and certain of the animal functions, induced Dr. Philip to inquire whether the analogy could not be extended, and whether, not only the contractility of the muscles, but the more characteristic effects, which he supposed to be necessarily dependant upon the nervous influence, might not be produced by galvanism. The secretion of the gastric juice appeared to offer an unexceptionable mode of putting his hypothesis to the test of experiment. For this purpose, after dividing the par vagum, a portion of the lower end of the nerves was coated with tin foil, and a silver plate placed over the stomach of the animal; the tin and silver were then respectively connected with the opposite extremities of the galvanic apparatus. It was found, by this arrangement, that the animal seemed to be entirely free from the various distressing symptoms which always, in a greater or less degree, attend the division of the nerves, and upon examining the contents of the stomach, after the

death of the animal, the food appeared to be perfectly digested, affording a complete contrast to what was contained in the stomach of a similar animal, in which the nerves had been divided, but which had not been subjected to the galvanic influence.¹

The singularity of the result, and the important consequences which were deduced from it, caused this experiment, like that of the division of the nerves, to be repeated by various physiologists, and although the facts were, in the first instance, warmly controverted, they appear to be now fully confirmed. Dr. Philip then proceeded to examine how far the other effects which he ascribed to the nervous influence, could be imitated by galvanism; and with this intention he made the experiments of which I have given an account in the last chapter,² on the evolution of heat from arterial blood by this agent. From the whole of his observations and experiments, taken in conjunction with each other, he conceived himself warranted in the conclusion, that every effect of the nervous influence might be produced by galvanism, and in short, that the two agents are, strictly speaking, identical.³

¹ See the original experiments in the "Inquiry," p. 223. . 237; ex. 70. . 4. Many experiments on the subject will be found in the references to note (8). A digested summary of the hypothesis is contained in a paper in the Quarterly Journ. v. viii. p. 72. et seq.

² See page 286.

³ For the correct conception of this hypothesis it is necessary to bear in mind, that Dr. Philip supposes what he stiles the nervous functions, to be confined to the four following operations,

According to Dr. Philip's view of the subject, we have two positions, the truth of which appears to be involved in the hypothesis of secretion which he supports, first, that the nervous influence is essential to this function, and second, that the nervous influence is identical with galvanism. As, however, these positions are in themselves completely distinct, it may be desirable to examine them separately, in order that we may more clearly comprehend the nature of the evidence on which they each of them rest. With respect to the first of these points, the dependance of the function of secretion upon the nerves, it appears to be the direct inference from the result of the division of the par vagum, and it must be admitted that the argument has been very forcibly urged by Dr. Philip, and that the reasoning which he employs, and the facts which he adduces in its support, are very impressive, and appear at first view almost unanswerable. Yet, perhaps, upon reflection, we shall be induced to think that the conclusion does not so inevitably follow from the premises, as has been supposed to be the case. The clear and legitimate inference from them is, that the nervous influence

"stimulating the muscles, both of voluntary and of involuntary motion, conveying impressions to and from the sensorium, effecting the formation of the secreted fluids, and causing an evolution of caloric from the blood;" *Inquiry*, p. 220. Perception and volition he denominates sensorial functions, supposing them to be more immediately connected with, or dependent upon the brain, as distinguished from the nerves. See the 10th chapter of the *Inquiry*; also *Quart. Journ.* v. xiii. p. 97, and v. xiv. p. 91, 3.

has a necessary connexion with the action of the glands, but in the particular case of the stomach it is not very easy to trace out the series of changes which takes place, in such a manner as to show how far the connexion is essential, or how far it is only what may be deemed incidental. In order to prove that the latter is the case, it is only necessary to bring forwards one unequivocal example of a secretion being produced, where there can be no intervention of nervous influence, but of this we have numerous instances in all the various classes of the lower tribes of animals, in which no nervous system has yet been detected.⁴ This I conceive to be so direct an argu-

⁴ Cuvier, Leçons, t. ii. p. 361.. 3; Lamark, Anim. sans Vert. t. i. p. 24, 5. Perhaps we may adduce, in this place, the cases that are on record of monstrous or deformed fœtuses being born, with many of their organs fully developed, in which the nervous system was entirely wanting; one of the most remarkable of these is that detailed by Clarke in the Phil. Trans. for 1793, p. 154. et seq.; the author fairly infers that this system cannot be essential to the functions of the fœtus, and to secretion among the rest. We may, however, conceive that the fœtus may possess some supplementary action derived from the mother. I have already had occasion to refer to Mr. Lawrence's valuable paper "on monstrous productions," where other cases are related analogous to the above; Med. Chir. Tr. v. v. p. 165, et seq.; in all these instances the secretions appeared to be produced as if the fœtus had possessed the natural and perfect structure.

Several cases of this description are also related in the different volumes of the Mem. Acad. Scien.; in the one for 1711, p. 26. there is a notice of a fœtus by Fauvel, where the cerebrum, cerebellum, and spinal cord were wanting; the writer remarks, that this case affords "une terrible objection aux esprits animaux," and we may add an equally forcible one to any hypo-

ment, as to be sufficient to counteract the force of any individual facts or experiments, however striking, which may be urged in favour of the contrary opinion. To assert that these animals have a nervous system, because they exercise those functions which have been generally supposed to be affected by means of the nerves, when no organs of this kind can be detected, and when the animals are of such magnitude as that their structure can be distinctly examined, is a mode of reasoning, which I conceive to be so palpably incorrect, as to require no formal

thesis which supposes the nervous system to be essential to what are usually termed the vital and natural functions. There is a notice of a case by Mery in the vol. for 1712, p. 38; a more ample account of another by the same, in 1720, p. 8. et seq.; and one by Winslow in 1724, p. 586. et seq., who also subjoins other cases to his paper. The first of Mery's cases, which was without either the brain or the spinal cord, is said to have lived for 21 hours, and to have taken food; the act of deglutition must of course have been performed. We have another remarkable case by Le Cat, of which there is a translation in the Phil. Trans. for 1767, p. 1. et seq.; although the whole of the upper part of the body was deficient, there appear to have been an imperfect spinal cord and cerebellum.

There is a well narrated case of this kind in the Phil. Trans. for 1775, p. 311. et seq. by Cooper; Hewson appears to have assisted at the examination; we are told, p. 314, that "upon a careful inspection there is evidently no brain or spinal marrow." I may remark, that Sir Ev. Home, in opposition to the authority of Cuvier and Lamark, as referred to at the commencement of this note, asserts, "that in the most simple animal structures endowed with life, large enough to admit of dissection, brain and nerves are met with;" Phil. Trans. for 1825, p. 257.

refutation.⁵ And, indeed, we can require no more striking illustration of a secreted fluid being produced without the intervention of the nerves, than the experiment itself which we are now considering; for we find that after the complete division of the nerves, when the action of the gland is either suspended, or at least altogether deranged, it may be restored and maintained, by the intervention of a physical agent, so far as appears, in its perfect and natural condition.

And, indeed, should it even be proved, that the nerves are essential to the secretion of the gastric juice, it would only tend to establish this particular fact, while we have so extensive a range of operations, in which secretion is always going forwards in situations or under circumstances where it appears impossible that the nerves can have any influence. The question, therefore, respecting the influence of the nerves in secretions, appears to be precisely analogous to that respecting the intervention of the nerves in muscular contraction, which was discussed in the fourth chapter.⁶ That the nerves are very frequently concerned in both the operations, is admitted, but the point to which we are to direct our inquiry is, whether there are any cases in which muscular motion and

⁵ The analogy of the vegetable kingdom may, I conceive, be fairly applied to the illustration of this question; for although we may regard its powers and functions as essentially different from those of animals, yet on this particular point they may admit of comparison.

⁶ V, i. § 4. p. 291. et seq.

secretion can be performed without the co-operation of the nerves; a single unexceptionable example of this kind, is sufficient to decide the controversy.⁷

We are now brought to the second of Dr. Philip's positions, the identity of the nervous influence and galvanism, and I think it must be admitted, that if we could prove this identity, some of the most formidable objections against the nervous hypothesis would be removed. And here, as in the former case, although I acknowledge that Dr. Philip's arguments are very clearly and ingeniously stated, and very happily enforced by his experiments, yet I conceive that there are some points in which they are essentially defective. The argument is, that we are able to imitate all the operations which properly belong to the nerves, by galvanism, and that therefore the two agents must be identical. But a moment's reflection will convince us, that the apparent similarity of two effects is not sufficient to prove the identity of the causes. The coagulation of albumen is produced by heat, by galvanism, and by various chemical re-

⁷ I may refer the reader in this place to a judicious essay by Dr. Alison, *Quart. Journ.* v. ix. p. 106, the object of which is to controvert Dr. Philip's hypothesis of secretion. The line of argument which I have employed in the text is somewhat similar to Dr. Alison's, but he has not always used the term nervous action or nervous influence in the same sense with Dr. Philip, which I have thought it necessary to do in discussing this question, whatever opinion I might be induced to form of its propriety or utility. See also Bichat "*Sur la Vie*," p. 244. 7, and some very judicious remarks by Mr. Shaw, *Lond. Med. Journ.* v. xlix. p. 454. et seq.

agents, yet we are not warranted in asserting that heat, galvanism, and all the chemical re-agents in question are identical. The sensation of sight is excited by light, by galvanism, and by a blow on the eye, but we do not suppose that light, galvanism, and mechanical violence are identical. We conclude, in these cases, that the first set of causes act in some way upon the albumen, and the second set upon the optic nerve, so as ultimately to affect the albumen and the optic nerve in the same manner. The mere similarity in the effect is therefore no proof of the identity of the causes, employing the term in its ordinary acceptation, the means which we make use of to produce the change in question, but of the minute and intimate operation of which we are frequently altogether ignorant.

But in the case under consideration it will be said, that we have not merely a single coincidence of an apparent cause and effect, but that there are two agents, one of which seems to produce all the characteristic effects of the other, so as to increase in a very high degree the probability of their identity. The force of the argument will therefore depend upon the number of these coinciding circumstances and the accuracy of the resemblance. In the case of the nervous influence, there are four of its characteristic effects pointed out by Dr. Philip,⁸ all of which are said to be capable of being imitated by galvanism, a circumstance which must add very great weight to

⁸ Inquiry, p. 116, 7; Quart. Journ, v. xiv. p. 91, 105.

the reasoning, provided the fact be clearly made out in each individual instance. In the first case, that of the production of the gastric juice, the resemblance of effect is undoubtedly very remarkable; but even here, I do not think that we are warranted in applying the same reasoning to all the other secretions, by an inference from that of the gastric juice alone. We are very little acquainted with the nature of the gastric juice itself, or of the mode of its operation upon the food, and although it is, upon the whole, highly probable, that the stomach is furnished with glands, which secrete a fluid that acts somewhat in the manner of a solvent upon the aliment, yet it is rather by inference, than by direct observation that we arrive at this conclusion.

And although we find that when the par vagum is divided, the food remains undigested, so as to render it probable that the gastric juice is not duly secreted, still we find that the surface of the stomach and the lungs pours a fluid, indeed probably in greater quantity than natural, although its physical and chemical properties are not the same as when the nerves are entire. The very circumstance of the anatomical structure of the par vagum, which renders them so well adapted for the experiment, seems to point out that there is something peculiar in their nature or their action, and that they either perform other functions besides that of secretion, or that there is some peculiarity in their secreting power, which is connected with their peculiar anatomical disposition.⁹

⁹ Analogy would induce us to suppose that if nervous action were necessary for secretion, it would be derived rather from

If we succeed in weakening the inference which is deduced from the first case of coincidence, that of the secretion of the gastric juice, I think the others will not be found sufficiently powerful to support the argument. I have already made some remarks on the experiments in which heat appeared to be evolved from arterial blood, by the application of galvanism;¹ and with respect to the power of this agent in exciting muscular contraction, when we consider in how many ways this effect is produced, we shall think it quite sufficient to abide by the old opinion, that galvanism, in these cases, acts as a subtle stimulus, without venturing to decide upon the nature of the operation.

With respect to the power of galvanism in conveying impressions to and from the sensorium, which it is stated to possess in common with the nervous influence, I do not perceive that Dr. Philip has advanced any direct facts in proof of this point. The power of exciting muscular contraction by transmitting the galvanic influence from the origin of a nerve to its extremity, or of producing an impression on the sensorium, than from the cerebral or spinal nerves. See the remarks of Mr. Shaw, as referred to above, p. 416. So far as the question can be elucidated by anatomy, we are informed that although numerous nerves go to the glands, they evidently pass through, or by them, to be ultimately distributed upon other parts; see particularly Haller, *El. Phys.* vii. 2. 2. He likewise informs us as the result of his experiments, that little sensibility is manifested by the glands, when different stimuli are applied to them; *Mém. Sur les Part. Irrit. et Sens.* t. i. p. 39. 59. P. 285.

sorium by transmitting the influence in the opposite direction, if these be the circumstances from which the conclusion is derived, would appear to prove no more than that galvanism is a stimulus both to the contractility of the muscles, and the sensibility of the nerves, a circumstance which is by no means peculiar to it, or characteristic of its effects.

Although the supposed improbability of an opinion should not be allowed to stand in opposition to the evidence of decisive and unequivocal facts that may be adduced in its favour, yet, in cases of this kind, we naturally require stronger evidence and more decisive facts than under ordinary circumstances. On this account, it is necessary to inquire into the antecedent reasonableness of any physiological doctrine, independently of the experiments or observations that are brought forwards in support of it, to ascertain how far it coincides with our ideas of the other operations of the animal œconomy.² The doc-

² Dr. Philip, in speaking of the distinction between the sensorial and nervous powers, uses the following expression: "However blended the organs of the sensorial and nervous powers may appear to be, we are assured that they are distinct organs, by the fact, that while the organs of the nervous power evidently reside equally in the brain and spinal marrow, those of the sensorial power appear to be almost wholly in man, and chiefly in all the more perfect animals, confined to the former;" *Quart. Journ.* v. xiv. p. 93. Upon the hypothesis of the identity of the nervous power and galvanism; the preceding remark must, I conceive, imply that there is in the brain and spinal cord, an apparatus provided for the accumulation or evolution of electricity, from which it may be transmitted, when required, along the nerves, and to which it again returns, when it conveys im-

trine of the identity of the nervous power and galvanism necessarily leads us to the conclusion, that the use of the nerves, as distinguished from the brain and spinal cord, is to conduct electricity, that all the operations which are supposed to be produced by the nervous communication of the different organs must be resolved into the conveyance of electricity from one to the other, and that when an impression is made upon the extremity of a nerve which is communicated to the sensorium, whatever may be its nature, it is effected by the intermedium of the electric fluid, and in like manner the same agent must be the medium of communication from the brain to the extremity of the nerves. When, for example, mechanical violence, or a chemical aerid, irritates the surface of the body, when light acts upon the retina, or the undulations of the air upon

pressions from the extremities to the centre. This idea seems to be farther countenanced by another expression in the same essay, p. 91., that there is "no evidence that impressions are ever communicated from one nerve to another, independently of the intervention of one of these organs" (the brain and spinal cord). I think it would be difficult to point out any structure in either of these parts, which can be supposed to be appropriated to this purpose, nor will it, I apprehend, be easy to show, why the currents should not as readily pass from one nerve to another, as in the course which we observe the nervous power to follow. It is often curious to observe some crude indications of the most recent, and apparently original doctrines, among the older authors. Sauvages says, that many experiments prove that the nervous influence is nothing more than electrical fluid, charged with some particles of extremely attenuated lymph; *Œuvres Div. t. ii. p. 233.*

the nerve of the ear, the pain excited, the visible impression produced, and the perception of sound are all conveyed to the sensorium by galvanism; and when we form a volition, and by means of it produce the contraction of a muscle, the will must act upon the galvanism which resides in the nerve that belongs to the part, and cause it to contract, while the same agent must again pass up from the muscle along the nerve to the brain, and communicate to it the perception or consciousness that the volition has been executed.

There is certainly a clear foundation for the distinction which Dr. Philip has laid down between the nervous and the sensorial powers, or the functions of the nerves and of the brain, although, perhaps, there are certain cases in which we may doubt, whether the line which he has traced out be, in every instance, quite correct. But admitting of the division in its fullest extent, it seems scarcely reasonable to conclude, that the actions of these organs depend upon totally different principles, that the nerves operate merely through the intervention of electricity, while it must be supposed that the brain can have no farther connexion with this agent, than to receive the impressions which it makes upon it. It would appear more natural to suppose that the mode of operation of the nervous system should be similar in all its parts,³ although, with certain modifications or

³ In making this observation, it is to be understood, that I do not mean to refer to the intellectual functions which are attached to the brain, but those which it possesses as a mere vital agent.

additions, each of them, as we presume, possessing every power or property of the other, together with what may be necessary for the exercise of those functions which belong to it exclusively. This community of properties might be expected to exist more particularly with regard to those nerves which are immediately connected with the brain or the spinal cord, which parts, from their connexion with each other, it is natural to suppose must possess, to a certain extent, the same properties. It has been found that the power of transmitting the galvanic influence, if not confined to certain nerves, is at least much more remarkable in some of them than in others, and this difference exists in so great a degree, that many eminent physiologists have been unable to excite any contractions in the nerves that are connected with the ganglia, and which are not under the control of the will. Yet if the nervous power be identical with galvanism, there appears to be no assignable reason, why these nerves should not be at least as sensible to this stimulus, as the nerves that belong to the voluntary organs, since they cannot be supposed to be deficient in the mere nervous functions, although they may not be possessed of those which are confined to the sensorium.

We have frequently had occasion to remark upon the great diversity in the nature of the substances which act as stimulants to the muscles; and although I have endeavoured to establish the doctrine of their independent contractility, yet, at the same time, it was shown that in a great number of in-

stances the stimulating substances act through the intervention of the nerves ; but if we are to regard the nervous power as identical with galvanism, it will follow that all these agents, mechanical, chemical, and vital, must operate through the medium of electricity, a supposition which appears quite repugnant to our ideas of their physical relations, and of the nature of electricity itself. And I may observe, that the difficulty, with respect to animals that are without a nervous system, is scarcely, if at all diminished, by supposing that the nervous power is identical with galvanism ; for we have the effects that are ascribed to galvanism produced without the existence of the nerves, which are supposed to be the channels through which this agent is conveyed. Should we go a step farther and maintain that galvanism is capable of being transmitted through every part of the body, as well as through the nerves, we may indeed elude the present difficulty ; but we become involved in the contradiction of supposing that nerves are necessary for the performance of a certain function, because they serve to convey the galvanic influence, yet that other parts of the body, as well as the nerves, are adequate to this conveyance.

Upon reviewing the subject of the theory of secretion, the inquiry, when considered in the abstract, may be stated as follows. We have a fluid possessed of certain properties, and consisting of certain components, from which various other substances are produced, the greatest part of them composed of the same elements with the primary fluid, but in different

proportions; by what means are these secondary substances formed? we may conceive of both chemical and mechanical agencies being concerned in these operations; if the substances produced are identical with any of the constituents of the primary fluid, or even very similar to them, it may appear probable that the operation is principally mechanical, whereas if the secondary substance differs considerably from any of the constituents of the primary fluid, we should naturally suppose that it has been produced by a chemical affinity, or by the combined effect of chemical and mechanical action. We must next inquire, how far the different modifications of chemical and mechanical actions, which may be conceived to exist, are sufficient to produce all the effects which we actually observe to take place, and to this inquiry we can only reply, that the present state of our knowledge on the subject of animal chemistry does not allow us to go farther than to say, that the changes which have been actually produced are very numerous, and that those which may be supposed possible are still more so, and that if there be any which we cannot explain, they will probably be equally difficult to account for upon any other principle. On this view of the subject, therefore, the question will rather be an appeal to our ignorance, than to any principle which can direct our judgment in deciding upon this point.

In the fourth place we must ask, in what manner are these supposed chemical or mechanical changes con-

nected with the operations of the living system; which of the vital powers are called into action, and through what medium are they excited? When we consider the mere act of secretion, to which our present inquiry extends, I should say that contractility is the only vital power which is essential to the operation. The action of the heart, in the first instance, propels the blood into the capillaries; it is transmitted through these with different degrees of velocity, and subjected to various modifications of compression, so that its constituents are more or less intimately mixed together; some of its finer parts are transmitted into vessels too minute to admit of those that are more viscid or tenacious, while, at the same time, it may be supposed to experience various alterations from changes of temperature, from the action of the atmosphere, or from the mixture of the different secretions with each other. Then, although for the reasons stated above, I conceive that the action of the nerves is not essential to secretion, it is sufficiently obvious that the organs of secretion, in the higher orders of animals, are very much under the influence of the nerves, and are, in many cases, materially affected by them, so that we are in possession of an additional agent, by which we may multiply the number of possible combinations of elements, and produce a corresponding number of new substances. Still, however, we are to bear in mind, that we can form no clear conception of any mode in which the nerves can act upon the organs of secretion, except

through the medium of the circulating system, so that here again we reduce the primary operation to the contractility of the muscular fibre, notwithstanding the share which the nervous system may have in the effects, considered as a secondary agent.

A great difficulty which remains to be obviated, respects the formation of the saline secretions, or of those substances, the elements of which are not to be found in the blood, or at least not in sufficient quantity to account for the great accumulation that takes place in certain parts of the system, and where we are unable to point out any means by which they can have access to it. This is a difficulty which, I confess, appears at present insurmountable, and it can be said, that it attaches equally to every hypothesis that has been proposed; for it is at least as difficult to say in what manner the action of the nerves should produce lime from the blood, as how it should be produced by any operations of chemical affinity. To suppose that we are affording any real explanation of the phenomenon by ascribing it to the operation of the vital principle, or to any vital affinities, which is merely a less simple mode of expressing the fact, is one of those delusive attempts to substitute words for ideas, which have so much tended to retard the progress of physiological science.

APPENDIX.

AFTER I had written the preceding chapter, I learned from Dr. Philip that a new edition of his "Inquiry" was in the press. From the conversations which we had on the subject, I conceived it probable that his hypothesis of the identity of the nervous influence and galvanism would be stated in a more clear and forcible manner in this new edition than it had been in the former; I therefore requested him to allow me to peruse that part of his work which referred to this point. From this I have drawn up the following remarks, which I think will be found to contain a faithful account of the arguments on which his opinion is founded.

The arguments may substantially be reduced to four; they are to be met with in the 13th chapter, entitled, "On the Nature of the Vital Powers." 1. He commences by the observation to which I have already alluded, that in comparing the sensorial and the nervous functions, the latter (using the word in the restricted sense in which it is always employed by Dr. Philip) are found to bear a strong resemblance to the physical, or, as he styles them, the inanimate powers of nature, while no resemblance of this kind can be traced with respect to the former. The acts of secretion and of calorification are analogous to many chemical processes, the transmission of impressions through the nerves to both chemical and me-

chanical processes, while the excitement of the muscular fibre is the effect of various physical agents. But, as the author remarks, we can trace no resemblance or analogy between sensation (perception) or volition, and the operation of any physical agents. Hence the deduction is made, that the nervous influence must depend upon a physical agent, as it appeared a necessary consequence, that where the phenomena bore a clear analogy to the effects of physical action, they could not be referred to a power which exclusively belongs to a living system.

2. The author next remarks that the vital functions must be supposed to be necessarily confined to the organization of the organs which are their peculiar seat, for that a power which is independent of the organ in which it resides cannot be regarded as a vital power. If it can exist in any unorganized or inanimate body, it must be regarded as a mere physical property. To apply this principle to the case under consideration, it is argued, that if the nervous influence be a vital power, it must be necessarily confined to a substance similarly organized with the nerves, and be incapable of existing in a part possessed of any other kind of structure. "If the nervous power can be conveyed by other parts, it is not a vital power, but one that may reside in unorganized bodies." Dr. Philip then proceeds to relate the experiments that were performed on the division of the par vagum, in which it was found, that when the division had been complete, and the divided ends of the nerves had even retracted for a small space, still the secretion

of the gastric juice was continued, and consequently, according to his hypothesis, the nervous influence must have passed through the interval, and of course have been conveyed through this space by the moisture or some other interposed body ; and hence, as it can exist attached to this body, it is deduced that it cannot be a vital agent. The author observes, as a circumstance that, at first view, caused some surprise, that this transmission of nervous influence between the divided extremities of a nerve, can only take place with those that are attached to the ganglions, for that he was unable to produce the same effects with the nerves from the spine. This apparent anomaly he endeavours to account for upon the principle that the action of secreting surfaces is increased by whatever produces an unusual determination of blood to them, which "solicits towards them a corresponding supply of the influence of the nervous power." But nothing of this kind takes place with respect to the cerebral or spinal nerves.

3. The third argument which Dr. Philip adduces in support of his hypothesis, is derived from the fact, that "we can substitute for the nervous power a variety of inanimate agents." The muscles can be excited by various mechanical and chemical stimuli, and also by the nervous influence, but as we here perceive that the nervous influence only acts the same part, or produces the same effect which may be produced by physical agents, it is concluded that this must likewise be a physical agent.

4. Dr. Philip having thus rendered it probable, or rather, as he conceives proved, that the nervous influence is a physical agent, he proceeds to inquire, whether it consists in something which is peculiar to the animal body, or whether it be an agent which operates in the production of other natural phenomena. It was supposed that electricity, or rather that modification of it which has been termed galvanism, was the most likely to possess the necessary requisites, and for the purpose of ascertaining how far this conjecture was sanctioned by the phenomena, the experiments were performed, of which an account has been given above. They consisted in applying galvanism so as to produce by means of it a secreted fluid, viz. the gastric juice, and to evolve heat from arterial blood; and as galvanism appears to have the property of acting as a stimulus to the muscular fibre, and of conveying impressions along the nerves to the sensorium, it is supposed that we are able to produce, by means of it, every effect of the nervous power, and hence Dr. Philip deduces an argument in favour of their identity.

The above remarks, as I conceive, afford a clear and faithful account of Dr. Philip's hypothesis, and of the arguments by which it is supported. I fully admit the force of many of the facts on which it is founded, and the ingenuity with which the author has deduced his conclusions from them, yet I must confess that they do not bring conviction to my mind. I have little to urge in favour of my own opinion,

in addition to what has been stated in the preceding pages; but I was desirous that an hypothesis of so much importance in physiology, and one brought forward by so powerful an advocate, should be fairly presented to my readers; and having done this I leave it to be decided by future investigations.

CHAPTER X.

OF DIGESTION.

THE last chapter contained an account of the means by which certain substances are separated from the blood, for the purpose of contributing more or less directly to the preservation of the constituents of the body in their perfect and healthy condition; I am now to describe the mode by which the loss thus occasioned is repaired, by which fresh materials are, from time to time, received into the system, and assimilated to it. This is accomplished by the function of digestion, which, when considered in its most extensive sense, may be defined the process by which aliment is made to undergo a succession of changes, so as to adapt it for the purposes of nutrition. Perhaps, in strict propriety, we ought to regard this process as consisting of several subordinate processes, each of which might be considered as a distinct function; but as they all appear to be different steps of the same operation, and subservient to one ultimate object, that by which food is converted into blood, it will be more convenient to describe the whole of them in connexion with each other.¹

¹ See Cullen's *Inst.* § 201. The term digestion, in its primary technical import, was intended to express the operation by which the aliment is macerated in the stomach, by a process supposed to be analogous to the digestions which are carried on in the laboratory. See Castelli, *Lexicon*, "Digestio." Per-

The first change which the aliment experiences is entirely of a mechanical nature, and consists in reducing it into that state of minute division, which may prepare it for the future changes which it is to undergo. In man and the mammiferous quadrupeds this is accomplished by the teeth. After the food is sufficiently masticated, it is received into the stomach, where both its physical and its chemical properties are changed, and it becomes converted into a uniform pultaceous mass, termed chyme. From the stomach it passes into the duodenum, where it is detained for some time, and undergoes a farther change in its properties, and where it is converted from chyme into chyle.² The process of chylica-

haps the most valuable and elaborate part of Magendie's work is that which treats upon digestion; he conceives it to be made up of 8 subordinate actions; 1. reception of the food, 2. mastication, 3. insalivation, 4. deglutition, 5. action of the stomach, 6. of the small intestines, 7. of the large intestines, 8. expulsion of the fæces; of these the 5th and 6th may be regarded as the essential operations; *Physiol. t. ii. p. 33.*

² The terms chyme and chyle are generally employed by the modern physiologists in the way that is stated above, but it does not appear that there is any thing in their etymology which would lead to this distinction, nor was it recognised by the older authors, or even by some of those of the last century, who appear to have used the words indifferently, or to have considered them as synonymous. The following examples may be adduced among the older physiologists, where chyle is spoken of as formed by the stomach; Willis de Ferment. C. 5. p. 16; Sylvius, Disput. Med. 1. Op. p. 1, 2. et Prax. Med. lib. 1. C. 7. Op. p. 117; Fabricius de Ventriculo, Op. p. 113, 4, and p. 139; Gulielmini, de Sang. not. § 37; Charleton, Œcon. Anim. Exerc. 2. de Chylif. § 4. Lower, de Corde, p. 204; Pitcairne,

tion may be regarded as the ultimate result of the action of the digestive organs; there is still, however, certain changes to be effected before the complete assimilation is accomplished. The next step consists in the separation of the chyle from the refuse matter with which it is combined, and the transmission of this separated matter through the lacteals into the blood vessels. It is poured into the trunk of the great veins, near their termination in the right auricle of the heart, and being mixed with the

El. Med. C. 5. de Econ. Anim. §. 1; Riolan, Ench. An. lib. 2. C. 23. p. 118; Bartholin, de Lacteis Thorac. Cap. 1. p. 3; see also Castelli, "chymus" and "chylus." The distinction between chyme and chyle is not recognised by Boerhaave, see Prælect. § 78. .95; he appears, indeed, not to contemplate any essential difference between the contents of the stomach and the duodenum, except what depended upon the mixture of bile and pancreatic juice with the latter. Haller's opinion upon this point I shall notice more particularly hereafter; but I may remark in this place, that he does not uniformly employ the terms in their modern acceptation; see Boerhaave, Prælect. not. 12, ad § 83, not. 9, ad § 87; Prim. Lin. § 635, 638, 717, 8. et alibi. I have not been able to ascertain who it was that first assigned to the words their present signification. Scæmmering applies the term chyme to the alimentary matter when it arrives at the small intestines; Corp. Hum. fab. t. vi. p. 306. .9. § 216. M. Magendie employs chyme to signify the substance formed in the stomach; Physiol. t. ii. p. 72, 81, 2. Mr. Brodie employs the terms in their ordinary acceptation: Quart. Journ. v. xiv. p. 342; and this is also the case with M. Chaussier, Art. "Digestion," Dict. des Scien. Med. compare p. 406 and 429; this article, although written in a very diffuse stile, contains much valuable information. See also Dumas, Physiol. t. i. ch. 10. p. 350, et seq.

blood, is finally assimilated to it in all its properties. The subjects of this chapter will be arranged under five heads; I shall first give a general description of the form and structure of the digestive organs; in the second place I shall make some remarks upon the nature of the various substances that are used in diet; next I shall examine into the successive changes which they experience, from their first reception into the stomach, until they are deposited into the blood vessels; in the fourth place, I shall give an account of the hypotheses that have been invented to explain the nature of the operations; and lastly, I shall notice some affections of the digestive organs, which are indirectly connected with their functions.

§ 1. *Description of the Organs of Digestion.*

The digestive organs,³ as they exist in man and the higher orders of animals, may be conceived to

³ The existence of a stomach, or of some organ equivalent to it, has been supposed, by most naturalists, to be necessary to animal organization, and even to afford a definite character by which animal may be distinguished from vegetable life; see Smith's *Introd. to Botany*, p. 5. It is accordingly stated by writers on comparative anatomy, that no organs are so generally present, in all kinds of animals, as those which serve for digestion; and it is indeed self-evident, that every being possessed of life, must have the means of receiving and assimilating to itself the matter which is subservient to its growth and nutrition; Blumenbach, *Comp. Anat.* § 82. Sæmmering expressly says, "animal ventriculi expers innotuit plane nullum;" *Corp. Hum. fab. t. vi. p. 229*. It appears, however, that there are certain animals of the inferior orders, and some of no inconsiderable size, which are not furnished with any receptacle for con-

consist of three orders of parts, each of which serves a distinct and appropriate purpose. The operation of the first is entirely mechanical, constituting the means by which the food has its texture broken down, or is sufficiently comminuted to admit of the full operation of the next process, which is more of a chemical nature. At the same time, however, that this mechanical operation is going forwards, the food is mixed up with the various mucous secretions, that are found in the different parts of the mouth, fauces, and gullet; these tend to soften the alimentary mass, and render it more easily divisible, while they may, perhaps, have some effect in promoting the subsequent changes which it is to experience. In man and the mammiferous quadrupeds, this mechanical process is effected by mastication, as performed by the teeth. I shall not think it necessary to enter into any description of these organs, any farther than to remark, that although there is a general resemblance between their form and situation, in the different classes of animals, yet upon a more minute inspection, we find that they differ very considerably from each other in these respects; and upon examining these differences, in connexion with the habits of the various animals, we shall find in all cases, that they are

tain food, and which, therefore, like vegetables, must be supposed to imbibe their nutriment from the surface of the body; see Lawrence's Blumenbach, note 1. p. 129. Mr. Abernethy, in his 6th lecture, p. 193, 4, gives a list of the animals of various classes, in which Hunter had examined the digestive organs.

adapted to that species of alimentary matter, which is the best suited to the digestive organs and other functions of the individual. In some animals the teeth are so disposed as to be evidently intended for seizing and lacerating animal food; others, on the contrary, are better fitted for cropping and triturating the parts of vegetables; in short, so great a correspondence has been traced between the disposition and structure of the teeth, and the general habits of the animal, that naturalists have not unfrequently assumed these organs, as among the most characteristic features, by which to form the basis of their systematic arrangements.⁴

The food, after a due degree of comminution in the mouth, is transmitted, by the act of deglutition,⁵

⁴ Linnæus, *Sys. Nat.* t. i. p. 16. et alibi; Shaw's *Zoology*, v. i.; *Introd.* p. vii. et alibi. For an account of the human teeth it will be sufficient to refer to the treatises of Hunter, Blake, and Fox; to Scemmering, *Corp. Hum. Fab.* t. i. p. 177.. 207. § 224.. 237; and to Monro *Tert.* v. ii. p. 8.. 28; for the comparative anatomy of the teeth, to Cuvier, *Lec. d'Anat. Comp.* No. 17. t. iii. p. 103. et seq.; for the soft parts connected with the process of mastication, to Boerhaave, *Prælect.* t. i. § 58.. 64; Monro's *Elem.* v. i. p. 495.. 508; Bichat, *Anat. Des.* t. ii. p. 663. et seq.; and Magendie, *Physiol.* t. ii. p. 46. et seq.

⁵ There is, perhaps, no part of the human frame which exhibits a more beautiful specimen of mechanism than the organs that are concerned in deglutition. Simple as the process may appear, it is in reality very complicated, and consists of a succession of individual actions, each of which produces an independent specific effect, yet so connected with the rest, as to attain the object in view in the most perfect manner. After the aliment has been sufficiently comminuted by the teeth, it is

down the œsophagus, into the stomach, a bag of an irregular oval form, which lies across the upper part

moulded into a suitable form by the muscles of the mouth and the tongue, and is transmitted by them to the pharynx, which is, at the same time, so disposed, as to be put into the best position for receiving the mass; while the same action of the parts also causes the epiglottis to close the passage into the larynx. The food being now received into the top of the œsophagus, its muscular fibres commence their contraction, which proceeding progressively from the higher to the lower part, gradually propels the mass into the stomach. For a more minute account of the process of deglutition and of the parts concerned in it, see Boerhaave, *Prælect. t. i. § 70. . 2*; he concludes his description with the following remark; “*tam operosa fit arte deglutitio, tot conspirantes organorum adeo multiplicium et concurrentium actiones huc requiruntur.*” Haller, *Prim. Lin. ch. xviii. § 607. . 621*; *El. Phys. xviii. 3. 21. et seq. and xviii. 4. 1. et seq.* According to Morgagni, we are indebted to Valsalva for much of our knowledge respecting the muscles which are concerned in deglutition; see Valsalva, *Op. t. i. epist. 9. tab. 5 and 6.* For a description of the œsophagus and its appendages, see Bell’s *Anat. v. iv. p. 40. . 4*; Sæmmering, *Corp. Hum. Fab. t. vi. p. 201. . 8. § 109. . 126*; Monro Tertius, on the gullet, *p. 1. . 5*; and *Elements, v. iii. p. 508. . 512*; Bichat, *Anat. Descrip. t. iii. p. 379. . 397*; Dumas, *Physiol. t. i. p. 341. . 253*, divides the act of deglutition into four stages, “*temps* ;” during the first, the alimentary mass is propelled towards the gullet; during the second, the cavity dilates and receives it; during the third, the mass passes into the pharynx; and during the fourth, it is transmitted down the œsophagus into the stomach. Magendie, *Physiol. t. ii. p. 54. . 67*, marks three stages; by the first, the mass passes from the mouth to the pharynx; by the second, it enters the œsophagus; and by the third, it is transmitted to the stomach. A view of the muscles concerned in deglutition, is contained in Albiquis, *Tab. 10, 11, 12*; Santorini, *pl. 6*, which although entitled to but little commendation as a work

of the abdomen, to which it gives the name of the epigastric region. The structure of the stomach may be considered physiologically as three-fold. A large part of its substance is composed of membranous matter, which determines its form and capacity; it is plentifully furnished with muscular fibres, constituting what is termed its muscular coat, and it is lined internally with a mucous membrane, which appears to be more immediately connected with its secretions. Anatomists have indeed differed considerably in the account which they have given of the number of what are termed the coats of the stomach, but the difference is, in a great measure, verbal. Those who have made the most numerous divisions, have pointed out as many as eight distinct textures; first, the peritoneal covering, below which are two strata of muscular fibres, one longitudinal, and the other circular; a cellular coat connects this with the more dense membranous expansion, called, according to the phraseology of the older anatomists, the nervous coat; below this is another cellular coat, within which is the villous or innermost coat. Considered physiologically, these may be all reduced to the muscular strata, and the internal mucous lining, with the membranous matter which lies between them. It would appear that the ultimate termination of the nerves and vessels of the stomach, so far as they can be actually of art, is probably a correct delineation of the organs which it represents, and this commendation may be justly bestowed upon Watt's "*Anatomical Chirurgical Views*," the execution of which is stiff and harsh.

traced, is on the external surface of the mucous or innermost coat, and it seems probable that this is the seat of the glands.⁶

Besides the mucous fluid which is poured out by the internal membrane, in the same manner with all

⁶ For a description of the stomach, its form, situation, structure, &c. the student may be referred to the following works: Fabricius de ventric. Op. p. 99..149; Willis, Pharm. Rat. sect. 2; Winslow, sect. 8. § 2. 43..71; Boerhaave, Prælect. t. i. § 73. cum notis; Haller, Prim. Lin. ch. xix. § 622..6, and El. Phys. xix. 1. 6..11; Blumenbach, Inst. Physiol. § 352..4; Fordyce on Digestion, p. 6..12; Sæmmering, Corp. Hum. Fab. t. vi. p. 209..228. § 127..147; Bertin Mem. Acad. pour 1760, p. 58. et seq.; Sabatier, Anat. t. ii. p. 288..2; and Boyer, Anat. t. iv. p. 332, who enumerate four coats, the membranous, the muscular, the nervous, and the villous; Bell, Anat. v. iv. p. 44. et seq.; Bichat, Anat. Descrip. t. iii. p. 397..415; Buisson, the editor of this volume, reduces the coats of the stomach to three, the serous, the muscular, and the mucous; Monro Tert. Elem. v. i. p. 515; and Outlines, v. ii. p. 115, 6; Fleming's Zool. v. i. p. 314. et seq.; in Bell's Dissect. pl. 4. we have an excellent view of the stomach, and its contiguous parts; we have also a very characteristic drawing of the stomach by Mr. Bauer, Phil. Trans. for 1821, pl. 4. With respect to Willis's description of the coats of the stomach, it is not a little remarkable to observe the influence of words upon opinions; although a person of much knowledge and judgment, and one who had particularly attended to the nervous system, he subscribes to the opinion of the old anatomists, that the sensibility of the stomach, and the other parts of the digestive organs, is seated in what was called the nervous coat; see Pharm. Rat. p. 6, 13. Among the older physiologists or anatomists, who have given us characteristic views of the stomach, and the parts connected with it, we may select Vesalius, De Corp. Hum. Fab. lib. v. fig. 10..19; Eustachius, Tab. Anat. No. 10. fig. 1, 2, 3; Ruysch, Thes. Anat. 2. tab. 5; Santorini, tab. 11.

other bodies of a similar texture, the stomach has been supposed to possess glands that secrete the peculiar fluid called gastric juice, which acts so important a part in the process of digestion; the existence, however, of any distinct glands for this purpose is rather inferred from the effects which it is supposed that they produce through the intervention of their secretion, than from our being able to demonstrate their existence.⁷ The membranous substance of the stomach appears to be peculiarly distensible, so as to admit of having its capacity much increased, while its muscular fibres give it a high degree of contractility, by which means its bulk is capable of being diminished as occasion requires, and is thus always exactly adapted to the quantity of its contents.

The muscular fibres being connected to the membrane either individually, or in small separate groups, not only enable the stomach to contract in its whole extent, and in all directions, but bestow upon its separate parts the power of successively contracting and relaxing, so as to produce what is termed its peristaltic, or, perhaps, more appropriately, its vermicular motion.⁸ The action of these fibres appears

⁷ See Haller, *El. Phys.* xix. 1. 14; Bell's *Anat.* v. iv. p. 58; Winslow says that the glands are situated on the internal surface of what is termed the nervous coat, and that there are perforations in the innermost coat, which afford a passage to the excretory ducts; *Sect.* 8. § 2; but it may be questioned whether this was the result of actual observation.

⁸ Haller, *El. Phys.* xix. 4. 9, 10; Boyer, *Anat.* t. iv. p. 333. . . 5; supposes the muscular fibres of the stomach to be disposed in three layers, the first being a continuation of those of the

to be so directed as to produce two mechanical effects; in the first place, the successive contraction of each part of the stomach, by producing a series of folds and wrinkles, serves to agitate the alimentary mass, and by bringing every part of it in its turn to the surface, to expose it to the influence of the gastric juice, while, at the same time, the whole of the contents are gradually propelled forwards from the orifice, which is connected with the œsophagus, to that by which they are discharged. These fibres, like all those which compose the muscular coats, are not under the control of the will.

There are few parts of the body which are more copiously provided with blood vessels than the stomach, a circumstance which is evidently connected with the great degree of vitality possessed by this organ.⁹ Its nerves are likewise very numerous, and are remarkable for the variety of sources whence they are derived. It not only partakes of the ganglionic nerves, which are thickly dispersed over it, in common with the neighbouring viscera, but it likewise derives a supply of nerves from the spinal cord, and is distinguished from all the other parts of the body, except what are termed the organs of sense, by having a pair

œsophagus, the next the transverse or circular fibres, and lastly, two large muscular bands, which are situated obliquely to each other. One of the first accurate descriptions that we have of the muscular coat of the stomach is by Bertin, *Mem. Acad. pour* 1760, p. 58. et seq.

⁹ Haller, *El. Phys.* xix. 4. 19; Blumenbach, *Inst. Physiol.* § 356.

of cerebral nerves, almost entirely devoted to it, although it is situated at so great a distance from the brain.¹ The specific uses of all these nerves will be more fully considered hereafter ; but I may remark, in this place, that the stomach appears to possess, in a very high degree, many of the powers which are ascribed to the nervous influence, it is exquisitely sensitive, while it partakes remarkably of the general actions of the system, and sympathizes with all its changes, so that it may be regarded as a kind of common centre, by which the organic functions are connected together, and their motions regulated.

The extremity of the stomach, by which the food is received, is termed the cardia, that by which it is discharged the pylorus ; the latter is furnished with a fold of the membranous coat, and also with a number of muscular fibres, possessing in some degree the property of a sphincter, so as to retain the food until it is in a proper state for being discharged,² and thus

¹ Winslow's Anat. v. ii. sect. 8. § 2. par. 78, 9 ; Haller, El. Phys. xix. 1. 21 ; Blumenbach, Inst. Phys. § 355. p. 201 ; Bell's Anat. v. iv. p. 64 ; he observes that the par vagum is distributed principally over the cardia, and that this appears to be the most sensitive part of the stomach. Of Walter's plates, Nos. 3 and 4, it is impossible to speak too highly ; the accuracy of the drawing, and the clearness of the engraving, are equally deserving of admiration.

² The peculiar office and functions of the pylorus is one of those subjects that was considered by the older anatomists as something singularly wonderful or mysterious. The delicate sensibility of the stomach was conceived to reside chiefly in this part, and it was also thought to produce some specific effect in the process of digestion, which could only be explained by sup-

resisting the vermicular action of the muscular coat, when it would tend to propel the aliment through the pylorus, before it had undergone the requisite preparation. The food is also prevented from being too quickly discharged by the relative situation of the cardia and the pylorus; as the stomach lies across the abdomen, these two orifices are nearly on the same horizontal line, and in consequence of the form of the organ, and the connexion of the neighbouring parts, whenever it is distended with food, a large part of its contents will be below the level of the pylorus,³ so that it must require a considerable force of muscular contraction in the stomach to discharge its contents, in which it is probably aided by the diaphragm and the abdominal muscles.⁴

posing it to be endowed with certain extraordinary powers and qualities. Vanhelmont conceived it to be the peculiar seat of the soul, an opinion to which Willis gives a degree of support.

M. Richerand ascribes to it something like intelligence, when he says that it has a peculiar tact, which enables it to select from the contents of the stomach what is proper to pass through, while it rejects the remainder; *Physiol.* § 23. p. 111, 2.

³ It has, however, been remarked by anatomists, that although it is the great curvature which principally becomes distended when food is received into the stomach, and that we are in the habit of considering this as the lower part of the organ, yet by distention, this part is protruded forwards as well as downwards, so that, perhaps, there may be no greater portion of the contents below the level of the pylorus than in the more contracted state of the organ. But it is probable that the mere power of gravity is but little concerned in the transmission of the food through the stomach. See Haller, *El. Phys.* xix. 4, 5.

⁴ Haller, *El. Phys.* xix. 4. 2, 3.

The intestinal canal,^a which receives the aliment when it leaves the pylorus, is a long winding cylindrical tube, varying much in its different parts, as to its form and diameter, but which, like the stomach, may be considered as essentially consisting of three structures, the membranous, the muscular, and the mucous, each of which, like the corresponding parts of the stomach, serves respectively to give the organ its general form, to impart to it a degree of contractility, and to furnish the appropriate secretions. The intestines have been divided by anatomists into the two great classes of small and large, a division which refers entirely to the diameter of the parts, but which may also be connected with certain specific differences in their form, structure, and situation. Each of the two portions is then subdivided into three parts, which, commencing with the stomach, have received the names of duodenum, jejunum, and ilium, for the small, and cœcum, colon, and rectum, for the large intestines.

The division into the small and large intestines, may be considered as founded upon their physiological nature, as well as upon their anatomical structure,

^a For a description of the form and structure of the intestinal canal, it may be sufficient to refer to Haller, *El. Phys.* lib. xxiv; *Monro Prim. Ed. Med. Essays*, v. iv. p. 76. .92, a paper which contains much important information respecting the minute anatomy of the parts; Blumenbach, *Inst. Phys.* sect. 28; *Bell's Anat.* v. iv. p. 70. .83; *Monro Tert. Elem.* v. i. p. 533. .550. We have an interesting account of the comparative anatomy of these organs in Blumenbach, *Comp. Anat.* note A, p. 177, and in Sæmmering, *Corp. Hum. Fab.* t. vi. p. 281, et seq.

for it appears to be in the former alone that any part of the digestive process is carried on, the latter being solely intended to remove from the system the refuse matter, which is incapable of undergoing the process of chylication. With respect to the small intestines there are no definite characters, either anatomical or physiological, by which the jejunum and ilium can be distinguished from each other; but the case is different with respect to the duodenum, the structure and functions of which are sufficiently appropriate.⁶

⁶ Haller proposes to denominate the whole of that portion of the small intestines, which lies behind the mesocolon, the duodenum, and to apply the term small intestines, *intestinum tenue*, to the remaining part, as the jejunum and ilium entirely agree in their structure and functions, and are obviously different from the duodenum in these respects. It is only partially covered by the peritonæum, it is firmly attached to the spine, not merely connected to it by a loose membrane, as is the case with the other parts of the small intestines, its diameter is larger, it is more vascular and glandular, its muscular fibres are stronger, and it has larger folds; the pancreatic and biliary ducts open into it. See Winslow's *Anat.* v. ii. sect. 8. § 3. par. 108. et seq.; Haller, *Prim. Lin.* ch. 24. § 719. not. ** ad § 96. in Boerhaave, *Prælect. and El. Phys.* xxiv. 1. 4, 5; also Bell's *Anat.* v. iv. p. 65.; Fordyce on *Digestion*, p. 15. 9; Monro 3^d. *Elem.* v. i. p. 534, where we have the description of the duodenum by Monro *Prim.*, copied, although with some variations, from the *Ed. Med. Essays*, v. iv. p. 66. 8; this essay is accompanied by a plate; see also Sandifort, *Tab. Duod.*; Sæmmering, *Corp. Hum.* *Fab. t.* vi. p. 283. 5. § 182. 5; Richerand, *Physiol.* § 25. p. 115, 6; Bichat, *Anat. Des. t.* iii. p. 416. 421; Santorini, *C. 9.* § 7. p. 166, 7. The duodenum has been named by some anatomists the *ventriculus succenturiatus*, or accessory stomach, as being the organ in which the process of chylication appears to be perfected; Claussen, de *Duodeno*, in *Sand. Thes. t.* iii. p. 273.

It appears, indeed, to be the part which is subservient to the important process of chylification, while the office of the jejunum and ilium is principally confined to abstracting the chyle from the residual mass; this is accomplished by its being gradually transmitted along their cavity, thus permitting the lacteals to absorb the nutritive part, as it is brought into contact with their orifices. The means by which the absorption is effected, will be considered in the following chapter.

In the course of this work, I have confined my attention for the most part, to the functions as they exist in man, and in the animals which the most nearly resemble him, with only occasional observations on comparative physiology. There are, however, some remarkable deviations from the ordinary form and action of the digestive organs, even among the higher classes of animals, of which I shall give a more particular description, not only in consequence of the interest which may be attached to them considered individually, but more especially, from the information which they afford us concerning the function of digestion generally, by noticing the peculiarities of their structure, and observing the relation which their several parts bear to the operations of the human organs. I refer to the compound stomachs of the ruminating animals, and to the strong muscular stomachs of certain birds.

§ 19, 0; see Sabatier, *Anat.* t. ii. p. 302, 3; Boyer, *Anat.* t. iv. p. 345; Chaussier, in *Dict. des Scien. Med.* v. ix. p. 429. 4; and Dumas, *Physiol.* t. i. ch. 10.

Many of the mammalia possess a stomach of a much more complicated structure, and possessed of a much greater variety of distinct parts, than that of man. These animals feed principally on the leaves or stalks of plants, which they take in large quantity; the food is swallowed, in the first instance, without much mastication, and is received into a capacious cavity, called *venter magnus*, or paunch, where it remains for some time, as if for the purpose of being softened or macerated. Connected with this is a much smaller cavity, which, in consequence of its internal coat being drawn up into folds, that lie in both directions, so as to leave between them a series of angular cells, has obtained the name of *reticulum*, or honey-comb. From this second stomach the food is again brought up into the mouth, in the form of a rounded ball, and is then masticated by the animal, until it is sufficiently comminuted, constituting the process of rumination, or chewing the cud. The mass, when duly prepared, is again swallowed; but it now passes by the first and second stomach, and is conveyed into the third cavity, called *omasum*, or *maniples*, distinguished by the broad folds or ridges of the inner membrane, which are disposed longitudinally, and differ from those of the *reticulum*, in not being crossed by others in the contrary direction; it is also of smaller size than any of the other cavities. From this the food is sent into the fourth stomach, named *abomasum* or *read*, which is of a larger size, although much less than the paunch, is of an irregular conical form, the base

being turned towards the omasum, lined with a mucous or villous coat, which is disposed into rugæ like those of the third, and appearing, in its structure and functions, to be most analogous to the simple stomach of man and the other mammalia.^s

^s We have a very complete account of the digestive organs of ruminant animals by Peyer in his *Mericologia*; they are described, as it appears, with great minuteness, accompanied with coarse, but expressive engravings. We have excellent views of the parts by Daubenton, in Buffon's great work, *Nat. Hist. des Anim.* t. iv. pl. 15. .8, and by Sir Ev. Home in *Phil. Trans.* for 1806, p. 362. :5, pl. 15 and 16; and *Lect. Comp. Anat.* v. ii. pl. 21. .25. See also Haller, *El. Phys.* xix. 1.2; and xix. 4. 15; and Cuvier, *Lec. Anat. Comp.* t. iii. p. 363. .6. Among the older physiologists we have a good description of the parts by Fabricius, in his treatise "*De Varietate Ventriculorum*;" Op. p. 123. et seq. The reader may consult with advantage Grew's work on the *Compar. Anat.* of the stomach, a treatise, which in a short compass, contains many valuable, and probably original observations, respecting the comparative anatomy of the digestive organs; also Glisson, *de Ventriculo*, Ch. i. § 9. .15, p. 123. .7. There are certain animals which appear to possess a kind of intermediate stomach, between the simply membranous receptacle, and the complicated structure of the ruminants. This is particularly the case with the horse, in which the two halves of the stomach possess an obviously different structure, the left side seeming to be intended merely as a reservoir for the food, while the right half is provided with the villous coat and the glandular apparatus to adapt it for the purpose of chymification; Bertin, *Mem. Acad. Scien.* pour 1746, p. 23, et seq.; fig. 2, Blumenbach, *Comp. Anat.* § 87, p. 133. and note C. p. 153. From the remarks of Prof. Monro it appears somewhat doubtful how far this structure exists in the human stomach, as has been supposed by some physiologists; *Outlines*, v. ii. p. 111. .5. Hunter informs us that the whale possesses

There is some doubt as to the effect which is produced by the different parts of this complicated apparatus, and as to the use which they serve in the economy of the animal. It is, however, pretty clear that the object of the first stomach is principally that of maceration, which is still further completed in the reticulum, that this cavity as well as the omasum contain secretions which are mixed with the aliment, which it may be presumed are more or less similar to the saliva, while it is in the abomasum that the proper digestive operation, that of chymification, is conducted.⁹ There has been much discussion concerning the final cause of this arrangement, or concerning the cause why the maceration and mastication of the food is effected in a different manner in these animals from what it is in those that, in other respects, the most nearly resemble them. The popular opinion is, that, from the nature of their food, the large quantity of it which these animals require for their support, and the consequent length of time which is necessary for its complete mastication, it was requisite that it should be more completely macerated, and be mixed with a greater proportion of the different mucous secretions, than is the case in the or-

four stomachs, which in their structure and appearance bear a considerable resemblance to the digestive organs of the ruminants; but it appears that they do not correspond in their uses, as in this class of animals the second cavity seems to be that in which chyme is produced; *Phil. Trans. for 1787, p. 410, 1.*

⁹ Hunter on the Animal Economy, p. 212, 3.

dinary process.¹ It has, however, been doubted how far this hypothesis can be maintained, as there are some of the ruminant animals, where the organs of mastication, as well as the general habits of the animal, would appear to be adequate to the preparation of the food by means of a simple stomach.² When animals that possess ruminant stomachs take in liquids, they are conveyed in the first instance into the second stomach, where they serve to macerate the food as it passes from the paunch, so as to prepare it for the process of rumination.³ While the young animal is nourished altogether by the mother's milk, it passes directly through the third into the fourth stomach, and it is not until they begin to eat solid food that rumination is established. It has been supposed that the act of rumination is under the control of the will, and that the animals possess a voluntary power of conveying the food at pleasure either into the first or the fourth stomach, and of returning it from the third stomach into the mouth.⁴

¹ It was supposed by some of the ancient anatomists, as it appears by Galen and Aristotle, that the use of this particular organization of the stomach, was to compensate for the deficiency of the incisor teeth, the materials of which are applied to the formation of the horns. See remarks upon this opinion by Fabricius, de Variet. Ventr. Op. p. 131, 2.

² Blumenbach, Comp. Anat. by Lawrence, p. 134. 8.

³ Home in Phil. Trans. for 1806, p. 363.

⁴ Grew, Comp. Anat. of the Stomach, &c. Ch. v. p. 26; Ray's Wisdom of God, &c. p. 275; Blumenbach, Comp. Anat. § 90, 1. p. 137, 8. The mechanism of these parts, as connected with

The other animals to which I alluded, as possessing a peculiar kind of stomach, are certain tribes of birds. Birds, although not provided with teeth, or any other organ of mastication, many of them feed upon hard grains or other substances, which the gastric juice does not seem to be capable of dissolving while in their entire state. To supply this deficiency, the birds who employ a diet of this description are provided with two peculiar organs, the crop or craw, *ingluvies*, and the gizzard, *ventriculus bulbosus*. The crop is a large membranous cavity,

each other, and their relative actions, are so curious, and exhibit so remarkable an example of mechanical contrivance, that I shall quote the account which is given of it by Blumenbach. "The three first stomachs are connected with each other, and with a groove-like continuation of the *œsophagus*, in a very remarkable way. The latter tube enters just where the paunch, the second and third stomachs approach each other; it is then continued with the groove, which ends in the third stomach. This groove is therefore open to the first stomachs, which lie to its right and left. But the thick prominent lips, which form the margin of the groove, admit of being drawn together so as to form a complete canal: which then constitutes a direct continuation of the *œsophagus* into the third stomach. The functions of this very singular part will vary, according as we consider it in the state of a groove, or of a closed canal. In the first case, the grass, &c. is passed, after a very slight degree of mastication, into the paunch, as a reservoir. Thence it goes in small portions into the second stomach, from which, after a further maceration, it is propelled, by a kind of antiperistaltic motion, into the *œsophagus*, and thus returns again into the mouth. It is here ruminated and again swallowed, when the groove is shut, and the morsel of food, after this second mastication, is thereby conducted into the third stomach."

attached to the lower end of the œsophagus, in which the food is received when it is first swallowed, and where it appears to be softened by the secreted fluids of the part. After a due degree of this kind of maceration, it is transmitted to the apparatus called the gizzard. This is a cavity of a moderate size and flattened spherical form, composed of four strong muscles. Two of these, which constitute the greatest part of its bulk, are of an hemispherical shape, of a peculiarly dense and firm texture, and lined internally with a thick callous membrane, of the nature of cartilage. Attached to these, forming, as it were, the ends of the cavity, are two other muscles, of much smaller dimensions, but of the same structure and consistence.⁵ There is an orifice which suffers the food to pass in small successive portions from the crop into the cavity of the gizzard, and the effect of the contraction of the two large muscles of this part is to move them laterally and obliquely upon each other, so that whatever is placed between them is

⁵ Blumenbach's *Comp. Anat.* § 99. Grew describes the gizzard as consisting of six muscles; four large ones which compose its principal substance, and two that are much smaller; *Comp. Anat. of the Stomach*, p. 34. These two latter are, however, only appendages to the gizzard, and serve to conduct the food into its cavity from the *bulbus glandulosus*. We have a good view of the whole apparatus, as it exists in the turkey, by Mr. Clift, accompanying a paper of Sir E. Home's in *Phil. Trans.* for 1807, pl. 5, fig. 1. See also *Lect. on Comp. Anat.* v. 2. pl. 49, 62. For some valuable observations on the action of the gizzard we are indebted to J. Hunter; *Anim. Econ.* p. 198, 9.

subjected to a very powerful combined action of friction and pressure. The force of trituration which these muscles exercise, is almost inconceivably great, so as not only to break down the hardest grains, and reduce them to a complete pulp, but even to grind to powder pieces of glass, and to act upon siliceous pebbles, and masses of metal, while, at the same time, the cuticular lining is so dense and impenetrable, as not to be injured by the introduction of lancets or other bodies with sharp cutting edges, which have been introduced into the cavity by accident or for the sake of experiment.⁶

The action both of the crop and the gizzard must be considered as essentially mechanical, the latter being equivalent to the teeth, and the former appearing to serve merely for the purpose of maceration. We always observe a strict connexion between the food of birds and the nature of their stomachs, those alone possessing the gizzard who employ substances which the gastric juice would not be able to dissolve in the entire state. The stomachs of carnivorous birds are termed membranous, in opposition to the strongly muscular organs which have been described above; these, however, are plentifully furnished with

⁶ For facts on this subject see Acad. del Cimento, p. 268, 9; Borelli, de Motu Anim. t. ii. prop. 189; Redi, Esperienze intorno a diverse cose, p. 89. et seq.; and Spallanzani, Dissert. i. § 5. 8 and 10. 22. Prof. Kidd has noticed a remarkable analogy between the digestive organs of the mole cricket, *Gryllus gryllotalpa*, and the stomachs of granivorous birds; Phil. Trans. for 1825, p. 222. 5, fig. 6, 7, 8.

muscular fibres, and possess the same kind of peristaltic and vermicular motion with the human stomach and those of the non-ruminant mammalia.

Most anatomists, in describing the muscular stomachs of granivorous birds, speak of the gizzard as analogous to the digesting stomach of man, or of the non-ruminant quadrupeds, that is, the organ by which chyme is produced, whereas, strictly speaking, it is merely a substitute for the organs of mastication. Grew, whose remarks on these parts are very judicious, although he seems to have considered the action of the gizzard as entirely mechanical, does not point out any provision for the production of chyme, probably because he considered trituration as alone competent to the process. He aptly describes the gizzard as a part, "wherein the meat, as in a mill, is ground to pieces, and then pressed by degrees into the guts in the form of a pulp. For which purpose the deductor serves to deliver the meat from the echinus to the laboratory, as a hepper to a mill. The four grinders, or chief operators, are the mill-stones." He then goes on to explain very correctly the mode in which the muscles act.⁷ The same remark applies to Peyer, who gives a full and correct account of the structure of the parts, and seems to consider the sole office of the crop and gizzard to be for maceration and trituration.⁸ Spallanzani, after proving, in the most decisive manner, that the action of the

⁷ Compar. Anat. of the Stomach, Ch. ix. p. 40, 1.

⁸ Anat. Ventr. Gall. in Manget, Bibl. Anat. t. i. p. 172. See also Haller, El. Phys. xix. 1. 7, and Fordyce on Digest. p. 172.

muscular stomachs is essentially mechanical, and that grains and other hard bodies are not digested when they are protected from the effects of trituration, proceeded to inquire how the triturated matter is converted into chyme, and seems to have established, that this, or any other soft substance, is acted upon by the gastric juice, as in membranous secreting stomachs. This fluid cannot be furnished by the gizzard itself, as its structure is evidently not adapted for secretion, but by a glandular apparatus which is situated at the lower end of the œsophagus.⁹

We may remark that birds, which have no organs of mastication, have no salivary glands, the secretions that are provided by the appendages to the stomach supplying the necessary fluids:¹ the *bulbus glandulosus*, or *echinus*, which is situated near the termination of the œsophagus, and which is found in almost all birds, is probably the supplementary part.² In this case, as in others of an analogous kind, besides the birds which have the stomach of a decidedly muscular, or of a decidedly membranous structure, there are many which have what may be termed intermediate stomachs; of these a copious list is given by Haller.³

⁹ Diss. No. i. § 3. . 8, 39. . 45, 52.

¹ Spallanzani, Exper. § 47. . 52, 79. Blumenbach's Comp. Anat. note I, p. 159.

² Blumenbach, p. 142; see also Grew, Comp. Anat. &c. Ch. 8; and Art. "Birds" in Rees.

³ El. Phys. xix. 1, 2. It is well known that granivorous birds are in the habit of swallowing small pebbles, a fact which seems to have been first noticed by the members of the Acad. del

It would appear probable that all the anatomical varieties in the structure of the stomachs of different animals may be resolved into their mechanical effects upon the aliment, as it seems that whatever be the nature of the food which is employed, if it be sufficiently comminuted or triturated, it is equally acted upon by the gastric juice. We find, indeed, that certain animals naturally confine themselves to certain kinds of food, and we must therefore conclude that such food is better adapted to the nature and constitution of the individual. But numerous ex-

Cimento; *Saggi de Esper.* p. 268. Notwithstanding the experiments of Spallanzani, § 27, 8, it appears that the food is not equally well digested without them, and we may easily conceive that they may contribute to the mechanical effect of the gizzard. Borelli, *de Mot. anim. par. ii. prop.* 192, 4, formed the extravagant idea, that these stones directly contributed to nutrition, an opinion which was opposed by Redi, who was aware of their real use; see *Esperienze*, p. 84, where he expressly says, "Quelle pietruzze sono come tante macinette raggirare da quei due forti et robusti muscoli de quali e composto ventriculo. . .;" also *Osserv.* p. 91, 2. Blumenbach, *Comp. Anat.* note 19, p. 145, 6, supposes that their especial purpose is to kill the grains, which, while alive, would resist the action of the gastric juice; but it is scarcely necessary to have recourse to this supposition. It has, however, been thought to receive some confirmation from the circumstance of the Pangolin, *Manis pentadactyla*, swallowing pebbles, for as its food consists of insects, which are not masticated, the pebbles have been supposed to be necessary for the purpose of crushing them, and thus depriving them of life, so as to render them more easily acted upon by the digestive fluids; p. 139. On the subject of these pebbles see also Hunter on the *Anim. Econ.* p. 196, 8; Fordyce on *Digestion*, p. 23, 4; Blumenbach, *Spec. Physiol. Comp.* p. 17.

amples are familiar to every one, where, either for the purpose of experiment, or from necessity, a total change has taken place, as for example, from an animal to a vegetable diet, or the reverse, without any apparent injury to the functions being produced, provided the mechanical texture of the food admits of its solution or minute division in the stomach.

§ 2. *An Account of the Articles employed for Food.*

The articles employed in diet may be classed under the two great divisions of animal and vegetable, each of them competent to the support of life, probably in all kinds of animals, although it would appear that, in most cases, one or the other is better adapted to the different species of them. From what has been stated above, it may be conceived, that this greater competency depends principally upon the mechanical properties of the substances, but they likewise differ considerably in their chemical nature, and this both with respect to their proximate principles and their ultimate elements. The ultimate elements of animal substances are oxygen, hydrogen, carbon, and nitrogen; vegetable substances contain oxygen, hydrogen, and carbon; but the proportion of carbon is generally greater, and of hydrogen less, while, for the most part they are either without nitrogen, or contain it in small quantity only.

Although there is reason to believe that every article of food which is received into the stomach, must experience a complete decomposition, and be assimilated into the state of chyme, before it can serve for nutrition, yet the successive steps of the

change, or the length of the process which it has to undergo, depends, in some measure at least, upon the similarity which there is between the alimentary matter and the materials of which the body is composed. We therefore find that carnivorous animals, in general, have less bulky and less complicated organs than the herbivorous, and that among the latter, those that feed upon seeds or fruits, with the exception of the ruminants, have them less so, than those which live upon leaves or the entire vegetables. The stomach and intestines of man assimilate him, in regard to the nature of his diet, more to the herbivorous than to the carnivorous animals,⁴ yet we find, as a matter of fact, that either kind of diet is perfectly competent to his nutrition and support, and that probably the best state of health and vigour is procured by a due admixture of the two classes of substances.

We find, indeed, that mankind are principally guided in the choice of their food, with respect to its animal or vegetable origin, by the facility with which they are able to procure either the one kind or the other.⁵ The inhabitants of the northern regions,

⁴ Cuvier, *Regne Anim.* t. i. p. 86. Lawrence's *Lect.* p. 217. et seq. The reader who is disposed to pursue this inquiry, may peruse the learned dissertation of Richter, "*de victus animalis antiquitate et salubritate*," where he will find the subject treated in the true spirit of German research.

⁵ See Haller, *El. Phys.* xix. 3. 3. The third section generally contains much useful and curious information respecting the different kinds of substances that have been employed in diet, either by nations or individuals; it is, however, liable to the imputation, from which many parts of this great work are not

where, at least during a considerable part of the year, vegetables could not be obtained, live almost entirely upon animal food, while in the warmer climates, where fruits and vegetables of all kinds are abundant, the diet is chiefly composed of these substances. We may remark, however, that this arrangement, although more a matter of necessity than of choice, is, on other accounts, the best adapted to their respective situations. An animal diet is probably better fitted for producing the vigour and hardihood of frame, which is requisite to brave the rigour of an arctic climate, while at the same time we may presume that it is more suited to the evolution of heat.

The proximate principles, or primary compounds of animal origin that are employed in diet, are fibrin, albumen, jelly, and oil, to which we may add sugar, osmazome, and some others of less importance. The animals that are employed in diet are taken prin-

exempt, of the references being rather numerous than select. See also Lorry, *Essai sur les Alimens*; Plenck, *Bromatologia*; Richerand, *El. Phys.* § 3. p. 83; Sœmmering, *Corp. Hum. fab.* t. ii. p. 241, 250. § 157. 161; Parr's *Dict. Art.* "Aliment;" Pearson's *Syn.* part 1; Lawrence's *Lect.* p. 201, 9; Thackrah's *2d Lect. on Diet*, p. 54. et seq. Dr. Stark collected a series of facts respecting individuals, who had lived for a considerable length of time on some peculiar kind of diet; *Works*, p. 94, 5. His experiments on the effect produced by different kinds of aliment upon his own system, which he pursued with unexampled perseverance, afford a number of very curious results, but it would be impossible to give any synoptical view of them, consistent with the elementary nature of this work; see *Journal*, p. 96. 168.

cipally from the mammalia, from birds, fish, the testacea, and the crustacea. The flesh of the mammalia and of birds consists chiefly of fibrin, together with a quantity of jelly united to it, especially in young animals. Milk, which from its destination as the food of the young animal immediately after birth, may be regarded as peculiarly adapted both for digestion and nutrition, consists of an emulsion of albumen, oil, and sugar, suspended in a large quantity of water. In the formation of cheese and butter, we abstract the greatest part of the water, and obtain the albumen and oil respectively in a state of greater or less purity according to the exact nature of the process which is employed. The eggs of birds, which likewise contain a peculiarly nutritive species of food, consist chiefly of albumen with a quantity of oily matter. Fish consist of a much greater proportion of albuminous and gelatinous matter, in some cases united with a considerable quantity of oil, and the same would appear to be the case with the testacea and the crustacea that are employed in diet. It is scarcely necessary to observe that the different kinds of soups consist nearly of the same proximate principles with the materials of which they are composed, a portion of the firm and dense substances being rejected, while the more soluble parts are dissolved, or, perhaps, rather suspended in the water, consisting therefore of fibrin, albumen, jelly, or fat, according to the age of the animal, or the part of it which is employed.

The vegetable products, which compose any considerable portion of our diet, are fruits, seeds, roots,

tubers, seed-vessels, stalks, and leaves. The most important of the proximate principles are gluten, farina, mucilage, oil, and sugar.⁶ In all those nations

⁶ Haller attempts to reduce all nutritious substances to one principle, jelly; *El. Phys.* xix. 3. 2; Cullen thinks, that the matter of nutrition is, in all cases, either "oily, saccharine, or what seems to be a combination of the two." *Physiol.* § 211. In his treatise on the *Mat. Med.* v. i. pt. 1. ch. 1. p. 218. et seq. he endeavours to show that acid, sugar, and oil, contain all the principles which contribute to compose the animal fluids. An account of the various articles employed in diet, is contained in the second chapter, p. 240. et seq. Fordyce also makes an unfortunate attempt at generalization, in reducing all the nutritious matter to mucilage; *Treatise on Digest.* p. 84; but as he admits of a farinaceous mucilage, a saccharine mucilage, &c. it is rather a verbal, than an actual inaccuracy; p. 91. . 107. In like manner he says that all the animal solids consist of mucilage and water; p. 86. The proximate vegetable principles mentioned in the text, as serving for nutrition, are those which are generally employed by the human species; it appears, however, that certain species of animals can extract nourishment from parts which are not capable of being digested by the organs of man; the beaver, for example, can digest the bark and wood of trees; *Blumenbach's Comp. Anat.* p. 139. Richerand attempts to show that the alimentary principle is, in all cases, either gummy, mucilaginous, or saccharine; *El. Phys.* § 3. p. 82. Dumas is disposed to regard mucus as the "principe eminentment nutritif," because, as he says, it forms the basis of our organs and of our humours; *Physiol.* t. i. p. 187; we may conclude, therefore, that by the term mucus he means albumen. Sir H. Davy, in the third of his lectures on agricultural chemistry, gives a concise view of the proximate principles of the vegetables that are ordinarily employed in diet; p. 73. et seq. M. Magendie classes all alimentary substances under the heads of farinaceous, mucilaginous, saccharine, acidulous, oily, caseous, gelatinous, albuminous, and fibrinous; *Physiol.* t. ii. p. 3, 4.

which have arrived at any great degree of civilization, the main bulk of the vegetable food is derived from seeds of various kinds, and particularly from some of the cerealea; of these wheat has always been held in the highest estimation. In some countries rice composes a large proportion of the food of the inhabitants, and in many of the warmer climates maize is largely employed.

Gluten has been considered as the best adapted for the purposes of nutrition of any of the vegetable principles, both in consequence of its being of easy digestion, and of its containing, in proportion to its bulk, the greatest quantity of nutriment. This circumstance depends upon its being the substance, the elements of which the most nearly resemble those of the animal kingdom, hence termed the most animalized of any of the vegetable principles, and this chiefly in consequence of the large quantity of nitrogen which it contains. It exists in the greatest proportion in wheat, while it is found in small quantity only in the other kinds of seeds, or in the parts of plants generally.⁷

⁷ We have a detail of the chemical relations of gluten, in Thomson's System, t. iv. sect. 19. It has been resolved by Sig. Taddie, into two proximate principles, which he has named gliadine and zimome; see Ann. Phil. v. xv. p. 390, 1. and v. xvi. p. 88, 9. An account of the original observations of Beccaria will be found in Bonon. Acad. Com. t. i. p. 123. et seq. Vogel examined two species of wheat which are cultivated in Bavaria, the triticum hibernum, and spelta; the former was found to contain 24 per cent. the latter 22 per cent. of gluten; Journ. Pharm. t. iii. p. 212.

Next to gluten, in point of importance as an article of nutrition, comes the farina; this is also found copiously in wheat and the other grains, and it likewise forms a considerable proportion of the nutritive parts of the various kinds of pulse and of tubers.⁸ The nutrition of leaves, stalks, and of seed-vessels, and the green parts of plants, resides in the mucilage which they contain, although, in most cases, this is united with a portion of saccharine matter, which materially contributes to their nutritive powers. Most fruits contain a basis of mucilage or farina, which is combined either with sugar or with oil. In the pulpy fruits, with the exception of the olive, the former chiefly prevails; they generally also contain a quantity of acid, in addition to their other ingredients, but it may be doubted whether the acid serves directly for the purposes of nutrition, or whether it should not be rather considered as indirectly promoting digestion, by its effect upon the stomach or the palate. The principal ingredients of the chesnut, which, in many countries, composes a large share of the diet of the inhabitants, are farina and sugar, while many of the nuts are composed of a

⁸ For an account of the chemical relations of farina, see Thomson's Chem. v. iv. sect. 17; Henry's Elem. v. ii. sect. 9; and Thenard, Chim. t. iii. p. 211. . 226. According to Braconnot, Ann. Chim. et Phys. t. iv. p. 383. rice contains 85 per cent. of farina; Vogel, Journ. Pharm. t. iii. p. 214. conceives the farina in rice to be as much as 96 per cent.; see also the analysis of rice by Vauquelin, *ibid.* p. 320.

basis of albumen, united to a quantity of sugar and oil.⁹

Sugar enters into the composition of many vegetable substances that are employed in diet, and although it is generally regarded rather as a condiment, than as a direct source of nourishment, yet it has been supposed to be the most nutritive of all the vegetable principles. Nearly the whole of the sugar that is consumed in Europe is produced from the sugar cane, the juice of which contains it in large quantity and in a state of considerable purity. Sugar is also procured from the root of the beet in considerable quantity, and in some parts of America from the sugar maple.¹ Oil, either animal or vegetable, is commonly employed, more or less, in diet, and is likewise conceived to be highly nutritive; in the warmer climates vegetable oil is principally used, whereas in the colder regions animal oil is employed, as procured from milk, in the form of butter.

⁹ See Boullay and Vogel on the analysis of the almond; Journ. de Pharm. v. iii. p. 337, 344.

¹ Dr. Thomson gives a list of the plants which contain sugar; Chem. v. iv. p. 31. There was a good deal of discussion among the older writers, respecting the nutritive properties of sugar; Lewis, Mat. Med. v. ii. p. 289, observes that in consequence of its property of uniting oily and watery bodies, it has been supposed by some to enable the unctuous part of the food to unite with the animal juices, while others have conceived, that, from the same cause, it will prevent the separation of the oily part of the food, and thus prevent it from contributing to nutrition; the author properly remarks, that experience has not shown that sugar can produce either of these effects.

There are two points of view in which the articles of diet may be contemplated, either as they are nutritive, or as they are digestible; the former respects their capacity of affording the elements of chyme; the latter, the power which the stomach has of causing them to undergo the necessary change. Between these there is an essential difference, and they do not, by any means, bear an exact proportion to each other. There are many substances, which appear to contain the largest proportion of the elements which constitute chyme, but which are by no means easily digested, and which are rendered more so by being mixed with other substances which are less nutritive.²

² This distinction is very clearly pointed out by M. Chaussier, in the art. "Digestion," Dict. de Scien. Med. an article which, although very diffusely written, contains much useful information. In the introduction to Spallanzani's work by Sennebie, we have a detail of a set of important experiments by Goss on the comparative digestibility of different kinds of alimentary substances. He had acquired the habit of swallowing air, which acted upon the stomach as an emetic, and thus enabled him to bring up its contents at pleasure, and to examine their state in the different stages of the digestive process; p. cxxxix., cxi. The experiments that have been adduced by M. Magendie, to prove that a proportion of azote is necessary for the support of animals, in which it was found that animals cannot live for any length of time upon pure sugar, oil, or gum, *Physiol.* t. ii p. 390. and *Ann. de Chim. et Phys.* t. iii. p. 66. et seq., I conceive prove no more than that the stomach is not capable of digesting these substances without some addition. Haller observes that certain animals are destroyed by the use of sugar, although to others it proves highly nutritive and salutary; *El. Phys.* xix. 3. 12. In Stark's experiments we have many examples of the indigestible nature of a diet composed of a single article, which

It seems probable that a certain quantity of what may be considered as merely diluting matter, tends to promote digestion, and that in this way, various substances, which when taken into the stomach alone are not competent to afford nourishment, become useful when combined with other substances.

When animals are in their natural state, we find that most of them uniformly adhere to the same kind of food, and this in a very remarkable degree. Thus we observe carnivorous animals feeding only on certain kinds of flesh, and among the herbivorous animals, only on certain plants, or even on certain parts of them, and there are many of the insects which would appear to be exclusively attached to single species of plants. This is not, however, the case with man; for within certain limits, his digestive organs are supposed to be kept in a more healthy state, by a due admixture of different kinds of food, than by any one article taken singly. Stark, who performed a series of experiments on the effect of different kinds of food

was easily digested when mixed with other substances; *Exper. on Diet*, in his works, p. 89. et seq. M. Magendie, *Ann. de Chim. et Phys.* t. iii. p. 76, refers to Stark's experiments on sugar, as conceiving them to prove the incompetency of substances that contain no nitrogen to the nutrition of the body; but I think that a reference to Stark's works will show that his health was as much affected by other articles which contained nitrogen in sufficient proportion. In order to render M. Magendie's experiments unexceptionable, it would be necessary to employ a diet which should be composed of a mixture of substances, all of them without nitrogen, as farina, mucilage, or gum, mixed with sugar or oil.

upon the stomach, which he pursued with remarkable perseverance and apparent accuracy, seems, in the results which he obtained, to have very clearly established the fact, that those substances which afforded the most nutrition, could not be used as the sole article of diet, for any length of time, without the stomach being deranged.

It is found also that there are great differences in the digestive powers of different individuals among the human species, some stomachs requiring a preponderance of animal, and others of vegetable food. There are many substances, which, although nutritious and salutary to certain individuals, appear incapable of being digested by others, and this is often the case, when it is very difficult to conceive to what ingredient we are to assign this peculiarity of effect. Much may often be ascribed to the effect of habit and accidental association, or even of mere caprice, but, upon the whole, we may conclude, that there are original variations in the powers of the stomach, which cannot be accounted for upon any other principle, either moral or physiological.

That this difference exists with respect to animals of different species, is sufficiently obvious. Besides the two great divisions of carnivorous and herbivorous, we find decided differences in each of the two classes. Among the carnivorous animals, we observe some feeding entirely upon the flesh of quadrupeds, some of birds, and others of insects. Among those that live upon vegetables, we likewise find that they have each their peculiar partialities for certain parts of

plants; the seeds, the fruits, the leaves, &c.; and we can frequently trace a manifest connexion between the substance on which they feed, and the structure of the teeth, so as to show that the selection is not the effect of accident or arbitrary choice, but that it is connected with the conformation of the body, and depends upon the permanent structure of the organs. With respect to the teeth, we meet with some that are manifestly adapted for seizing and biting, others for tearing or lacerating their prey; some that are more proper for cropping the succulent and delicate parts of plants, others for the protracted mastication of those that are more firm and dense in their texture. The beaks of birds are infinitely diversified in their form and structure; some long and pointed, some broad and flat, and others hooked or curved, so as to be most clearly fitted for the reception of certain kinds of food only; and we find, in all cases, that the nature of the stomach, whether membranous, muscular, or ruminant, whether simple, as consisting of one cavity only, or compounded of several, precisely corresponds to that of the teeth, and to the other organs and habits of the individual.

Liquids of various kinds constitute an important part of diet. These may be arranged under two heads;³ first, the different decoctions or infusions,

³ Boerhaave, *Prælect.* t. i. § 56; Haller, *El. Phys.* xix. 3. 20. 6; Scëmmering, *Corp. Hum. Fab.* t. vi. p. 250., 2. § 162. Magendie, *Physiol.* t. ii. p. 6. arranges drinks under the four classes of water, vegetable and animal infusions, fermented liquors, and alcoholic liquors.

either animal or vegetable, where various substances are dissolved or suspended in water, and where the specific properties of the liquid depend almost entirely upon that of the substance which is added to the water, or where we employ liquids for the mere purpose of quenching thirst, or enabling the stomach to act more readily upon the aliment. On the first class of substances, which are, as it were, intermediate between solids and fluids, I have already had occasion to offer some remarks. Their properties may be considered generally as very similar to those of the substances which compose them; being necessarily formed of the most tender and succulent parts alone, they are, in most cases, proportionably nutritious and digestible, yet there are certain constitutions or states of the system, in which the quantity of the fluid necessarily introduced appears unfavourable to the process of digestion. This may be conceived to operate in various ways; it may act by unduly distending the stomach, so as to interfere with the vermicular motion, or, in consequence of its bulk, it may stimulate the muscular fibres to contract too rapidly, and expel the food before it has undergone its appropriate change; it may, perhaps, dilute the gastric juice, or from the consistence and temperature of the alimentary mass, a tendency to fermentation may be induced, or to some other chemical change, which may interfere with the regular and healthy action of the organs. The same remarks, with respect to its action upon the stomach, apply to milk, which, strictly speaking, should be classed among the nutritive fluids; but the

farther prosecution of this inquiry belongs rather to the province of the physician than the physiologist.

Among drinks, properly so called, the most important is water, which is, perhaps, not only the best substance for quenching thirst, but is, with a few exceptions, the vehicle of all the rest. These may be classed under the two heads of vegetable infusions and fermented liquors. Of the former, those that are the most commonly used in Europe, are coffee and tea, both of which afford a useful and salutary beverage, when not taken to excess, which, although not themselves directly nutritive, seem to render the stomach more capable of digesting its contents.

Fermented liquors, of some kind or other, we find to be employed by all people that have made any considerable advance in the arts of life, and in civilization; in this country, they are principally made from barley, by means of the sugar which is evolved during the process of germination; in the other parts of Europe, the juice of the grape is principally employed; and in many parts of the world, various saccharine and mucilaginous juices are used for the same purpose. These liquors, like the vegetable infusions, if not taken of immoderate strength, or in undue quantity, are grateful and salutary, and seem to promote digestion, while they are likewise themselves, to a certain extent, capable of affording nutrition, in consequence of the portion of undecomposed sugar and mucilage which they generally contain. I think, however, we can scarcely extend this indulgence to distilled spirits, for, although they are occasionally valuable as medical

agents, they must be always more or less pernicious, when made habitual articles of diet.

A third class of substances remains to be noticed, such as are not in themselves nutritive, but which are added to our food for the purpose of giving flavour to it, stiled condiments. These are very numerous, and derived from very different sources, but they may be all reduced to the two heads of salts and spices. Their selection appears to depend upon very singular habits or even caprices, so that those substances which are the most grateful to certain individuals and classes of people, are the most disagreeable, or even nauseous, to others. It may be laid down as a general principle, that such articles as are, in the first instance, disagreeable to the palate, are those for which we afterwards acquire the strongest partiality, and which even become necessary for our comfort; whereas the frequent repetition of flavours that are originally grateful, is very apt to produce a sense of satiety, or even of disgust. The examples of tobacco, garlic, and assafoetida, on the one hand, and of such substances as possess simple sweetness on the other, may be adduced in proof of this position.

There is such a very general relish for sapid food, among all descriptions of people, and in all states of civilization, as to induce us to suppose that besides the mere gratification of the palate, some useful purpose must be served by it, and that it must contribute, in some important manner, to the digestion of our food. This peculiar kind of taste, with a few exceptions, does not seem to exist among the inferior

animals, who generally prefer the species of food, which is best adapted to their organs, in a simple state. Perhaps, this may be, in some measure, explained by the consideration that man differs from other animals in his capacity of existing in all climates and in all situations, and that, in order to enable him to do this, it was necessary that he should be omnivorous, as it is termed, that is, able to digest any substance, which affords the elements of nutrition. But, as in the process of chymification, it appears that all the aliment received into the stomach must be reduced to a mass nearly uniform in its constitution, it is reasonable to suppose that some assisting or correcting substances may be requisite to reduce the various species of aliment to one uniform standard. Vegetable substances, for example, when reduced to a soft pulp, and macerated at the temperature of the stomach, would probably have a tendency to the acetous fermentation, which we may presume will be corrected by the addition of aromatics and spices, whereas a mass of animal matter may be prevented from degenerating into the putrid state by salts or acids. Both these articles seem to be originally agreeable to the palate; and as a general rule, we shall find that they are naturally produced in the regions where they are the most required. The different species of brute animals exist in those countries alone where there is a supply of food adapted to their digestive organs, and as they are guided in their choice of
is of diet by instinct, they do not require
these correctives.

There is, however, one remarkable exception to this general rule, in the relish which many animals of the higher orders seem to have for salt. Besides the various kinds of fish that inhabit the sea, and the birds that feed upon marine animals, we find that many quadrupeds and land birds clearly indicate their fondness for salt, and we have daily examples presented to us of its salutary operation, when either incidentally or intentionally mixed with the food of animals. Many singular instances are mentioned of the extraordinary efforts which they often make to procure it, when it is otherwise difficult to obtain. The beasts of prey that inhabit the central parts of the African and American continents, are known to travel immense tracts for the purpose of visiting the salt-springs that are occasionally met with, and it is said that these springs have been in some instances discovered by means of their footsteps, and by the hovering of birds over them. At the same time that we thus find animals to be led by instinct to the use of salt, we perceive that the human species are induced to employ it from its grateful effect upon the palate; for it may be remarked, that among all the singular diversities of tastes that exist among nations and individuals, there are no people, from the most barbarous to the most refined, who do not relish a certain portion of salt in their food.⁴ It may, perhaps, be thought not an unreasonable conjecture, that as salt always

⁴ Haller, *El. Phys.* xix. 3. 11; Fordyce on Digestion, p. 55. I believe that the opinion which appeared to be established by the experiments of Pringle, that although salt is powerfully antiseptic under ordinary circumstances, it promotes the decom-

exists in the blood and the other fluids, and must therefore be supposed to be of some essential use in the animal œconomy, there is a provision made for a regular supply of it in the constitution of our organs, and in our natural propensities and instincts.

There is a numerous class of substances, which are somewhat analogous to the condiments in their effects upon the stomach, although very different in their action upon the palate, the various medicaments. These do not afford nutrition, but they many of them tend to put the stomach into a state which adapts it for the digestion of aliment, and they produce upon the system generally, or upon some of its organs, certain changes, of which we take advantage in correcting its diseased actions and restoring its powers, when perverted or weakened. The farther prosecution of this subject does not fall within the province of the physiologist, but I may observe that the operation of medicines affords us many interesting examples of the nature of the vital functions, and conversely, that a correct knowledge of these functions must very materially contribute to guide us in the selection and administration of these substances. I may remark, that condiments and medicines differ in one essential circumstance from the articles of diet; that whereas the latter are always resolved into their ultimate elements, before they contribute to nutrition, the former act in their entire state, and if decomposed, would probably cease to produce their apposition of alimentary matter, when added to them in small quantity, Appendix, p. 351, 2, is now generally supposed to be without foundation.

appropriate effects. Some of the substances which possess the most powerful action over the system are derived from the vegetable kingdom, and are therefore composed of the same elements with our ordinary articles of food, only combined in different proportions; and even the most active mineral or metallic substances become inert when they are resolved into their elementary constituents.⁵

There is perhaps no substance whose operation on the animal œconomy is more violent than the hydrocyanic acid, yet this is entirely composed of carbon, hydrogen, and nitrogen. The acrid extracts and the narcotic alkalies that are procured from vegetables, all consist of different proportions of oxygen, hydrogen, and carbon, to which, in some cases, a quantity of nitrogen is added. The pure metals appear to exert no action upon the system, although many of their oxides and salts are so acrid. Phosphorus differs from other bodies of the same class, in being more active in its simple, than in its compound state. The three elementary bodies, chlorine, iodine, and fluorine, which agree in many of their chemical relations, resemble each other also in their powerful action on the living body, and this violence of action they retain both in their simple and in their compound form.

What we commonly term poisons, are so denomi-

⁵ Fordyce observes, that certain insects live entirely upon cantharides, yet their fluids are perfectly mild; *On Digestion*, p. 86; also that the poison of the rattle-snake is perfectly innocent when taken into the stomach; p. 119.

nated in consequence of the popular conception of their effect upon the system, but in reality, they do not essentially differ from medicaments. The very powerful operation which they produce, when under due regulation, is, perhaps in every instance, capable of being converted to some salutary purpose, and is only noxious, when carried to an excessive degree.

§ 3. *Changes which the Food undergoes in the process of Digestion.*

I proceed, in the third place, to describe the successive changes which the food experiences, from the time when it is taken into the mouth, until it is converted into perfect chyle.⁶ The first necessary step

⁶ Dr. Prout, in a very valuable paper, which is inserted in the 13th and 14th vols. of *Ann. Phil.*, describes in succession the different stages which the aliment experiences, from its reception into the stomach until it is finally converted into blood. He divides the whole process into the four stages of digestion, chymification, chylification, and sanguification, which he conceives are performed respectively by the stomach, the duodenum, the lacteals, and the pulmonary blood vessels. Many of Dr. Prout's observations are original and of undoubted accuracy, while the remarks which he offers upon the facts are highly ingenious and deserving of great attention; but I think it is to be regretted, that he has occasionally employed a different nomenclature from that generally adopted, and one from which I do not perceive that any particular advantage is to be derived. He, for example, employs the word *chyme*, not to signify the mass into which the aliment is converted in the stomach, but "that portion of the alimentary matter found in the duodenum, which has already, or is about to become albumen, and thus to constitute a part of the future blood." It must be

is a due degree of mechanical division, in order to prepare the aliment for the chemical changes which it is subsequently to undergo; this division, as has been observed above, is accomplished in various ways, according to the structure of the parts concerned, either by mastication, trituration, or maceration, or by a union of the three operations. After the re-

observed that the term *albumen* is likewise employed after the example of Berzelius in a somewhat unusual sense, to designate the substance which may be regarded as the first rudiments of the blood, being applied collectively to its three principal ingredients, the serum, fibrin, and globules, in their incipient state. A minute, and as it would appear, a correct description of the appearances which the aliment presents from the action of the gastric juice is given by Dr. Philip; *Inq. Ch. 7. Sect. 1. p. 140. 155.* The principal facts which he states are, that the new and old food are always kept distinct from each other, the former being in the centre of the latter; the food is more digested the nearer it is in contact with the surface of the stomach; it is the least digested in the small curvature, more at the larger end, and still more in the middle of the great curvature; the state of the food in the cardiac differs from that in the pyloric portion; in the latter it is more completely digested and more uniform in its consistence; it is also more compact and dry in this part; it appears that the act of digestion is principally performed at the large end of the stomach, that the mass is gradually moved forwards to the small end, becoming more digested as it advances; we may presume that the secretion of the gastric juice principally goes on at the large extremity, and that its chemical action on the aliment takes place in this part, whence it is slowly propelled to the pylorus. It is accordingly the great end of the stomach which is found to be digested after death by the action of the gastric juice upon it. See also Magendie's remarks on the formation of chyme; *Physiol. t. ii. p. 81, 2.*

quisite mechanical change, the food is transmitted into the proper digestive stomach, and is there reduced into the soft pultaceous mass, termed chyme. It has been stated in a general way, that the specific odours, and other sensible properties of the alimentary matters employed, are no longer to be recognized in the chyme, but that whatever species of food be taken into the stomach, the resulting mass is always the same.⁷ As far as respects the same kind of animal, when the nature of the food employed is not very dissimilar, and all the functions are in a perfectly healthy and natural state, the position may be admitted, but it is not true to the extent in which the assertion has been made, for we learn from experiment that the chyme produced from vegetable matter, differs sensibly from that of animal origin, and it can scarcely be doubted, that in the different states of the stomach of the same individual, even where the same kind of food has been used, the chyme does not always possess precisely the same qualities. But this remark may probably apply to the stomach only, when its actions are deranged, in which case these deviations must be regarded as partaking of the nature of disease, and consequently affording no indication of the natural state of the function.

Although the properties of chyme have been frequently examined, and various experiments performed upon it, still there is considerable obscurity respecting the nature of the process by which it is formed, and

⁷ Haller, *El. Phys.* xix. 4. 24, 31.

we are not able to account satisfactorily for the effect which is produced. The operation may be considered as analogous to the effect of a proper chemical action, where the body is not merely divided into the most minute parts, and has its aggregation completely destroyed, but, at the same time, acquires new chemical properties. That this kind of solution of the food takes place is proved by the experiments of Reaumur, Stevens,⁸ and Spallanzani.⁹ They in-

⁸ *De Alimentorum Concoctione.* Stevens took advantage of a man who had been in the habit of swallowing stones, and afterwards rejecting them from the stomach. He caused this individual to swallow hollow metallic spheres, perforated with numerous orifices, and filled with different kinds of alimentary matters, c. 12. Ex. 1..9; he afterwards pursued the experiments upon dogs; he caused these animals to swallow the perforated spheres, and after some time destroyed the animals and examined the state of the aliment; Ex. 11..23.

⁹ *Exper. sur la Digest.* The experiments of Spallanzani on this subject are quite decisive, and so varied and multiplied, as to meet every objection that could be urged against them. He was perfectly indefatigable in the pursuit of truth, and he presents us with the rare example of a philosopher who errs rather from the excess than the deficiency of the experiments which he adduces in order to establish a point, which might often have been as satisfactorily proved by a smaller number. The principal object of the first of his dissertations is to show, that in granivorous birds, the action of the gizzard is necessary in order that the gastric juice may act upon the hard and un-masticated food which usually composes their diet. In the second he illustrates the action of the gastric juice in animals that possess what he styles intermediate stomachs; in the third, fourth, and fifth, he extends his observations to the membranous stomachs of the amphibia, fish, various quadrupeds, and lastly of man.

closed different alimentary substances in balls, or in metallic spheres, or tubes, that were perforated with holes, or in pieces of porous cloth; these were introduced into the stomach, and after being suffered to remain there for a sufficient length of time were withdrawn, when it was found that the inclosed substances were more or less dissolved, while the substance containing them, whether metal or cloth, was not acted upon, thus proving that the effect was not of a mechanical, but entirely of a chemical nature.

The opinion which is commonly entertained respecting the production of chyme, and the one which appears to be sanctioned by our experiments is, that the glands of the stomach secrete a fluid of a peculiar kind, which is named gastric juice,¹ that this

¹ For an account of the gastric juice see Boerhaave, *Prælect.* § 77. et seq.; Haller, *El. Phys.* xix. 1. 15 and xix. 4. 20; in the former of these authors we have a copious list of physiologists and chemists, who have procured it and examined its properties. Among others, the student may consult Reaumur, *Mem. Acad. pour 1752*, p. 480, 495; Fordyce on *Digestion*, p. 62; Hunter on the *Anim. Econ.* p. 214, 5; Blumenbach, *Inst. Phys.* § 357; Bell's *Anat.* v. iv. p. 58. et seq.; Richerand, *El. Phys.* § 20. p. 105. 6; Scemmering, *Corp. Hum. Fab.* t. vi. p. 266. § 173; Cuvier, *Leçons*, t. iii. p. 362. 6; Magendie, *Phys.* t. 2. p. 199; Monro, *Tert. Outlines*, v. ii. p. 119. 122, and *El.* v. i. p. 527. et seq. Spallanzani made it the subject of very numerous experiments, *Exper.* § 81. et seq. 145, 185, 192; the only properties which he detected in it were that it was slightly salt and bitter; see also the analysis of Scopoli in the same work, § 244, which seems to have been made with much accuracy; his conclusion is, that the gastric juice con-

acts as a solvent for the food, and that the chyme is a solution of the alimentary matters in this juice. This has been supposed to be proved by direct experiment. For, besides the facts that were ascertained by Spallanzani, respecting the solution of the alimentary substances that were inclosed in the tubes, he procured the gastric juice from the stomachs of various animals, sometimes by causing them to vomit after fasting, and, perhaps, still more effectually, by introducing sponges, which after some time were withdrawn, and the fluid pressed from them. Although in this way the glands of the stomach would probably experience an unusual excitement or irritation, which might be supposed to affect the nature of the secretion, yet the fluid which was procured by this means was found to act upon various alimentary matters very much in the manner which we conceive the gastric juice to do. When they were digested with it for some time, at a temperature equal to that

tains water, an animal substance, "*savoneuse et gelatineuse*," muriate of ammonia, and an earthy matter, similar, as he says, to what is found in all animal fluids. See also Dr. Prout in *Ann. Phil.* v. xiii. p. 13. Senebier gives us an account of various experiments that were performed by Jurine and others on the use of the gastric juice in healing wounds and ulcers; *Journ. de Phys.* t. xxiv. p. 161. et seq.; but it is unnecessary to remark upon the uncertainty of such experiments. We have in the same paper an account of Carminati's analysis of the gastric juice; he found it salt and bitter, and frequently acid; he points out its antiseptic properties, which are the most powerful in carnivorous animals, and appear to be connected with its acidity; it is also said to contain a little volatile salt, and a larger quantity of a muriatic salt; p. 168. et seq.

of the human body, a kind of imperfect chymification was found to be produced, perhaps as nearly resembling the natural process as could be expected from the imperfect manner in which the experiment was necessarily performed. This imperfection attaches both to the mode in which the gastric juice was procured, and also to the want of the vermicular action which belongs to the living stomach, by which, during the whole period of the process of digestion, the aliment is continually in motion, fresh portions of it being exposed to the action of the solvent, and the whole gradually pushed forwards from the cardia to the pylorus.

It is, however, not a little remarkable, that when the gastric juice has been examined with relation to its chemical properties, nothing has been detected in it which appears adequate to the effects we observe to be produced. As far as we can judge, it resembles saliva, or the ordinary secretions of the mucous membranes, substances which indicate no active properties, and which we are disposed to regard as altogether very inert in respect to their action upon other bodies. From some recent experiments of Dr. Prout we learn, that a quantity of muriatic acid is present in the stomach during the process of digestion, but there does not appear to be any evidence of the existence of this acid before the introduction of the food into the stomach, so that we may rather infer that it is, in some way or other, developed during the process of digestion, than that it is the efficient cause of it. After it is developed we may

indeed conclude that it essentially contributes to the completion of the process, although we are not, I think, able to explain the mode in which it operates.²

² We have many accounts among the earlier physiologists of acid being detected in the stomach during digestion; it was one of the essential doctrines of Vanhelmont, that an acid is developed by the action of the stomach upon the aliment, in what he styled the first digestion; *Ortus Med.* p. 164..7. et alibi; also Willis de Ferment. Op. t. i. p. 25; this acid, however, was generally supposed to depend upon a morbid condition of the organs. Haller says, that in its recent and healthy state, the chyme is neither acid nor alkaline; not. ad § 77. Boer. Præl. t. i. p. 138; and *El. Phys.* xix. 1. 15; but he afterwards informs us, that in some experiments, which were made at his request by Rastius, a tendency to alkalescency was detected in it. He classes the acescency of the stomach, along with fermentation, putrescence, and rancidity, under the title of "*Corruptio Varia*;" *Ibid.* xix. 4. 29. Fordyce also states, as the result of his own experiments, as well as of Hunter's, that the contents of the stomach are not necessarily acid; on *Digest.* p. 150, 1. We have a well written anonymous essay in Duncan's *Med. Com.* v..x. p. 305. et seq., to show that the stomach does not contain any acid. Spallanzani makes the presence of acid during digestion a distinct object of inquiry, and decides, that although it is occasionally present, it is not essentially so; *Exper.* § 239..245; See Hunter's remarks, in *Anim. Econ.* p. 293. et seq. We have a curious case related by Circaud in *Journ. Phys.* t. liii. p. 156, 7, of an individual who lived many years with a fistulous opening into the stomach; the contents appear to have been generally acid. M. Dumas relates a series of experiments which he performed upon this subject, the results of which led him to conclude, that the gastric juice is not necessarily acid, but that it occasionally becomes so by the use of certain aliments that are disposed to go into the acid state; *El. Phys.* t. i. p. 278..0. MM. Tiedemann and Gmelin seem always to have detected an acid in the digestive organs; *Ed. Med.*

But notwithstanding this uncertainty about the nature and operation of the gastric juice, many circumstances prove that the stomach, or rather the fluid which it secretes, possesses a proper chemical action, because its effect upon bodies submitted to it bears no proportion to their mechanical texture or other physical properties.³ Thus the secretion from the stomach acts upon dense membrane and

Journ. v. xvii. p. 457. Dr. Prout's experiments are in Phil. Trans. for 1824. p. 45. et seq.

³ M. Montegre has performed a series of experiments, which appear to have been executed with care and assiduity, but which lead us to an opinion respecting the nature of digestion, very different from what has been generally adopted. He possessed the faculty of being able to vomit at pleasure, and in this way obtained the fluid from his stomach for the purposes of examination and experiment. He conceives that what has been supposed to be the gastric juice is in fact nothing more than saliva, that it possesses no peculiar power of acting upon alimentary matter, that the principal use of the gastric juice is to dilute the food, and that the only action of the stomach consists in "une absorption vitale et elective," in which the absorbent vessels, in consequence of their peculiar sensibility, take up certain parts of the food and reject others. *Exper. sur la Digestion*, p. 20. et seq.; see the general conclusions in p. 43, 4. That the conclusion of M. Montegre respecting the action of the stomach cannot be maintained, is correctly observed by M. Berthollet, in his report upon the memoir, because, until the food has undergone a certain chemical change in the stomach, the substance does not exist which is taken up by the absorbents; and further, that whatever may be our opinion respecting the nature of the gastric juice, or the mode in which it operates, it has been unequivocally proved that it can act upon substances which are not in contact with the stomach, provided they are exposed to its secretions; p. 53, 4.

even upon bone, reducing them to a pulpy mass; while, at the same time, many bodies of comparatively delicate texture, as the skins of fruits and the finest fibres of flax or cotton, are not, in the smallest degree, affected by it. This selection of substances so exactly resembles the operation of chemical affinity, and is so directly contrary to what would be the effect of mere mechanical agency, as to prove beyond a doubt, that a chemical action takes place in the stomach, and we are only induced to hesitate in giving our assent to this opinion, because the qualities of the agent appear to be so unlike what we might expect to find in a body capable of producing such powerful effects.

Besides the property of dissolving the aliment and reducing it to the state of chyme, the gastric juice produces two other effects which are decidedly of a chemical nature, the coagulation of albuminous fluids⁴ and the prevention of putrefaction. It is as difficult in this case as in the former to explain how these properties can be attached to such a substance as chemical analysis would indicate the gastric juice to be. It is upon the coagulating power of the gastric juice, as residing on the surface of the stomach, that the method of making cheese depends. What we term rennet consists of an infusion of the digestive sto-

⁴ Fordyce on Digest. p. 57, 9; 176. et seq. Fordyce, to show the extent of the coagulating power, remarks, "the six or seven grains of the inner coat of the stomach infused in water gave a liquor which coagulated more than a hundred ounces of milk." See Prout, Ann. Phil. v. xiii. p. 13. et alibi.

mach of the calf, and by adding this to milk, we convert the albuminous part of it into the state of curd, which is formed into a dense mass by pressing out the more fluid part from it. But here again the difficulty occurs which was alluded to above, that we are at a loss to conceive, by what property in the gastric juice it is that this coagulation can be effected. The quantity of the rennet which is sufficient to produce the effect is so extremely small that it is difficult to imagine, how so minute a portion of a substance, which seems to possess no properties of an active nature, can produce such very powerful effects. I think that the present state of our knowledge will not enable us to explain the difficulty, but it must be remembered that the process of coagulation is itself one of the most mysterious operations in chemistry, that it is produced by a variety of substances, the action of which we are not able to reduce to any general principle, and the nature of which it is perhaps impossible satisfactorily to explain.

The other peculiar effect of the gastric juice, its antiseptic power, is no less difficult to explain than its coagulating property, yet there is no doubt of its existence, for it was distinctly ascertained by Spallanzani and others,⁵ that in carnivorous animals,

⁵ Experiences, § 250..2. et alibi; Hunter on the Anim. Econ. p. 204. M. Montegre does not admit the existence of this antiseptic property; Sur la Digestion, p. 21. et alibi; his experiments would likewise deprive it of its coagulating power, of which, I conceive, it is impossible to doubt. Since the above was written, I find that the experiments of Mr. Thackrah

who frequently take their food in a half putrid state, the first operation of the stomach is to remove the fœtor from the aliment that is received into it. The same power of resisting putrefaction is also manifested in experiments that have been made out of the body, in which it was found equally efficacious in resisting putrefaction, or even in suspending the process when it has commenced. The antiseptic power of the gastric juice is perhaps still more extraordinary and inexplicable than its coagulating property; we can only say concerning it, that it is a chemical operation, the nature of which, and the successive steps by which it is produced, we find it difficult to explain, at the same time that we have very little in the way of analogy which can assist us in referring it to any more general principle, or to any of the established laws of chemical affinity.

The conclusion then to which we are reduced is, that when the food has undergone a sufficient degree of maceration and mastication, or other mechanical process by which it is reduced to a state of sufficiently minute division, the fluids that are secreted from the surface of the stomach act upon it, so as to produce a complete change in its properties. This is analogous to a chemical change in all its relations, but it is remarkable from the difficulty which we have in con-

have led him to deny the antiseptic property of the gastric juice; *Lect. on Digest.* p. 14; but with every feeling of respect for this author, I may remark, that it would require a considerable number of negative results to set aside the force of the positive statements which we possess on this subject.

ceiving how so considerable an effect can be produced by so apparently inert a substance. During the process of chymification heat is occasionally extricated, and not unfrequently gas is evolved;⁶ both of these

⁶ The nature of the gases that are found in the different parts of the alimentary canal has been examined by Jurine and Chevreul. Jurine's results showed, that as we recede from the stomach, the proportion of oxygen and carbonic acid decreases, while that of nitrogen increases; and that the proportion of hydrogen is greater in the large than in the small intestines, and less in these than in the stomach; Mem. Roy. Med. Soc. t. x. p. 72. et seq. M. Chevreul analyzed the gas in the stomach, the small, and the large intestines, with the following results:—

In the Stomach.

Oxygen.....	11
Carb. acid.....	14
Hydrogen.....	3.55
Nitrogen.....	71.45
	<hr/>
	100.0

In the Small Intestines, in three different Subjects.

Oxygen.....	0.0	0.0	0.0
Carb. acid.....	24.39	40.0	25.0
Hydrogen.....	55.53	51.15	8.4
Nitrogen.....	20.08	8.85	66.6
	<hr/>	<hr/>	<hr/>
	100.00	100.00	100.0

Gas in the Great Intestines of the first of the three Subjects.

Oxygen.....	0.0
Carbonic acid.....	43.5
Carburetted hydrogen with a trace of sulphuretted ditto.....	5.47
Nitrogen.....	51.03
	<hr/>
	100.00

however would appear not to be necessary steps in the process, but the consequence of a morbid state of the function. Previous to the experiments of Dr. Prout, the generation of acid in the stomach was regarded in the same point of view, but we are now led to conceive that it is a necessary part of the digestive process, and that it contributes, in some way or other, to the formation of chyme. It still remains to be inquired, whether in all cases of what is termed acidity of the stomach, the acid formed be the muriatic, in what exact stage of the process it is pro-

Of the Second.

Oxygen.....	0.0
Carbonic acid	70.0
Hydrogen and carburetted hydrogen	11.6
Nitrogen	18.4
	<hr/>
	100.0

In the Cæcum of the Third.

Oxygen	0.0
Carbonic acid	12.5
Hydrogen	7.5
Carburetted hydrogen	12.5
Nitrogen	67.5
	<hr/>
	100.0

In the Rectum of the Same.

Oxygen.....	0.0
Carbonic acid	42.86
Hydrogen	0.0
Carburetted hydrogen	11.18
Nitrogen	45.96
	<hr/>
	100.00

Magendie, Phys. t. ii. p. 85; 104, 5, 112, 3.

duced, from what source the acid is immediately derived, and how it is ultimately disposed of.

I have already given some account of the vermicular motion of the stomach, or that alternate contraction and relaxation of the muscular fibres, by means of which its different parts are successively brought into action, which is propagated in a regularly progressive manner along the whole of the organ. This motion is evidently intended to mix all the parts of the alimentary mass intimately together, and to apply each portion in succession to the surface of the stomach, so as to bring it into contact with the gastric juice. It is principally produced by the contraction of the circular or transverse fibres, while it is probable that the longitudinal fibres have more effect in propelling the contents of the stomach from the cardia to the pylorus.

When the contents of the stomach enter the duodenum they are subjected to a new action, and undergo a farther change in their constitution and physical properties, by which the chyme is converted into chyle.⁷ The nature and properties of chyle have

⁷ From the remarks that were made above, p. 434, it will appear that the older physiologists were not thoroughly aware of the distinction between *chyme* and *chyle*, and still less were they acquainted with the exact part of the digesting organs in which the latter made its appearance. From the expressions which are employed by Boerhaave; Prælect. § 90. 5; it may be inferred, that he supposed the chyle was formed in the stomach, and was merely separated in the duodenum from the residual mass. Haller's opinion on this point is not stated with his usual clearness and precision; although, upon the whole, it

been examined with more minuteness and accuracy than those of chyme, although with respect to the mode of its production we are still less able to offer any satisfactory information. We seem to be in possession of little more than the general fact, that the contents of the stomach, soon after they pass into the duodenum, begin to separate into two parts, a

appears probable, that he supposed the process of chylification is not perfected in the stomach, but he does not make that distinction between the contents of the stomach and the duodenum which have been pointed out by later physiologists. See *Prim. Lin.* § 635, 8, 717. v. ii. p. 160, 1, 221, 2; also *El. Phys.* xviii, 4, 24, 31. and xxiv. 2. 1. Juncker very accurately discriminates between chyme and chyle; he says that the aliment is reduced to chyme in the stomach, and is then propelled into the duodenum, where it is converted into chyle, by the mixture of other substances with it; *Conspect. Physiol.* Tab. 11. De Secretione, and Tab. 25. De Nutritione. Vanhelmont complains that "*Usus duodeni neglectus in scholis*," and makes the following observation: "*Mirum dictu, quod aridus cremor in duodeno, salis suporem confectim acquirat, suumque salem acidum in salem salum, adeo libenter commutet.*" *Ortus Med.* p. 167, 8. On this, as on many other occasions, we shall find the opinions of Baglivi more nearly correct than those of his contemporaries; *Diss.* 3. Circa Bilem; *Op.* p. 429, 0. Nor have the moderns been always sufficiently correct on these points. See particularly Hunter on the Anim. Econ. p. 213. Fordyce, indeed, distinctly states, "that chyle is not formed in the stomach," yet in many parts of his treatise he argues as if the process of chylification was completed in the stomach. See Bell's *Anatomy*, v. iv. p. 65. et seq., and Monro's (Tert.) *Elem.* v. i. p. 552; where the fact is correctly stated, that the chyle is not perfected until the alimentary mass arrives at the duodenum. Richerand observes that the essential part of the process does not take place in the stomach, yet his expression would

white matter which constitutes the chyle being detached from the mass, while this, which consists of the residual matter, is converted into fæces, and finally rejected from the system. At the same part of the intestinal canal where this separation takes place, the

almost lead us to suppose that he conceived the action of the duodenum was merely to separate the nutritive from the excrementitious part of the aliment; *El. Phys.* § 14. p. 98. Sir E. Home, in his observations upon the purposes which are served by the different cavities in the whale, speaks unequivocally of chyle being formed in the proper digesting stomach, and appears not to be aware of the distinction between chyme and chyle; *Phil. Trans.* for 1807, p. 98, 9; the same remark applies to the paper in the second part of the same volume. With respect to this latter paper I may remark, that it contains a very interesting account of the formation of the stomach, which is traced through the various classes of animals, the object of which is to show that, in all cases, the cardiac and the pyloric ends differ in their structure, and consequently in their functions, the former serving, as would appear, more for maceration, the latter for chymification; the whole being illustrated by a series of excellent engravings. Sir E. Home states, that this diversity in the structure of the two parts of the stomach is marked by a visible contraction in the external form of the organ, owing to a transverse muscular band: p. 170. *et seq.* It is a little singular that he should suppose that it had not been noticed before, as we learn, by a copious list of references in *Monro's Elem.* v. i. p. 519, O; that it was described by Cowper, and has since been frequently referred to; to these references we may add *Haller*; *El. Phys.* xix. 4. 5; where it is expressed in the clearest manner, as having been seen both by himself and others. *Dr. Prout*, in his valuable paper to which I have so frequently referred, very clearly points out the specific operation of the duodenum; *Ann. Phil.* v. xiii. p. 12. *et alibi.* *Boer. Præl.* § 105; *Haller, El. Phys.* xxiv. 2. 3.

duodenum receives the secretions from the biliary and pancreatic ducts; and it is stated, that the chyle assumes its most perfect form, or is produced in the greatest quantity about the orifices of these vessels; so as to indicate some connexion between the process of chylification, and the action of the bile and pancreatic juice. We have, however, no very direct proof of any action taking place between these fluids and the chyme, so as to convert it into chyle, nor are we able to offer any explanation of the nature of the effect which they might be supposed to produce upon it. The pancreatic juice appears to be, in all respects, similar to the saliva, and although the bile might be supposed to possess more active properties, in consequence of the resinous matter and the soda which it contains, we have no data by which we are able to ascertain what their operation would be upon the chyme. It has been supposed by some physiologists, that the duodenum secretes a specific fluid, which converts chyme into chyle, as the gastric juice converts aliment into chyme; but, as we have no proof of the presence of this fluid, except its supposed utility in the case under consideration, it would be incorrect to assume its existence, without more direct proof. We appear then to be reduced to the conclusion that the change is probably affected either by the intervention of the bile and pancreatic juice, or the action of the constituents of the chyme upon each other; both these causes may operate, although perhaps the former is the more efficient of the two.⁸

⁸ We are indebted to Mr. Brodie for a series of experiments

The chyle, when thus procured from chyme, and separated from the residual mass, is a white opake substance, considerably resembling cream in its aspect and physical properties.⁹ When removed from the body it soon begins to concrete, and finally separates into two parts, a dense white coagulum and a transparent colourless fluid, an operation which appears to be very analogous to the spontaneous separation of the blood into the crassamentum and the serum. The chemical properties of the constituents of chyle appear also to be considerably analogous to those of the corresponding parts of the blood, and the chyle is likewise found to resemble the blood in the nature of its salts; but on the other hand, it differs from it essentially in containing a quantity of oil or fatty matter, an ingredient which is only occasionally found in the blood. Hence it appears, that in its chemical

which he performed on living animals, in order to ascertain the effect of bile upon the process of chylification, which are so simple, that, at first view, they appear decisive in favour of the opinion that the formation of chyle is the immediate result of the mixture of bile with chyme. They consisted merely in applying a ligature round the duct which conveys the bile from the liver into the duodenum, the result of which is stated to have been, that the process of chylification was suspended. But although the experiments must be regarded as highly interesting, I think we should be scarcely justified in giving our unqualified assent to the conclusion which the author draws from them, without a more full detail of the circumstances attending them than we at present possess; Quart. Journ. v. xiv. p. 341. et seq.

⁹ Fordyce on Digestion, p. 121; Young's Med. Lit. p. 516. fr. Berzelius; Dumas, *Physiol.* t. i. p. 379. 1; Magendie, *Physiol.* t. ii. p. 154. 8.

properties, as well as in its physiological relations, we may regard the chyle as a kind of intermediate substance between the chyme and the blood.

For the chemical analysis of chyle, we are principally indebted to Vauquelin, Marcet, and Prout,¹ who, in succession, examined it with much minuteness. Vauquelin employed the chyle of a horse, as obtained both from the thoracic duct, and from one of the principal branches of the lacteals. The chyle from the thoracic duct, when it had spontaneously coagulated, contained a clot which was of a light pink colour, its colour being deeper than that of the serous part, while the clot from the lacteals was almost white. The properties of the liquid part were very nearly similar to those of the serum of the blood; like this it contains uncombined alkali, but it differs from it in containing an oily or fatty matter, which is also found, although in smaller quantity, in the coagulum. The coagulum contained a basis of a substance considerably resembling fibrin, so as to indicate that the coagulum of chyle is of an intermediate nature between albumen and perfect fibrin.² The principal object of Marcet's experiments was to compare the chyle, as produced by vegetable and animal food in the same kind of animal; for this

¹ Emmert made some experiments on chyle, which he procured from the lacteals soon after it enters these vessels, but we do not obtain much precise information from them; *Ann. Chim. t. lxxx. p. 81. et seq.*

² *Ann. Chim. t. lxxxi. p. 113. et seq.; Ann. Phil. v. ii. p. 220. et seq.*

purpose he procured it from the thoracic duct of dogs. In all the essential points his results agreed with those of Vauquelin. The chyle consisted of a coagulum of a pinkish appearance, containing fibres or filaments, and of a fluid part very similar to the serum of blood, except that, in the animal chyle, there was the oily or fatty matter, which floated on its surface like cream. The vegetable chyle generally bore less resemblance to blood than that derived from animal food; the latter was more disposed to become putrid, and upon the addition of potash, it evolved a quantity of ammonia, which was not the case with the vegetable chyle, while the oily matter was found in the animal chyle alone. The two species were of the same specific gravity, and contained the same weight of saline matter, but the solid residuum of the animal chyle, as obtained by evaporation, was considerably greater than from the vegetable chyle. When they were both submitted to destructive distillation, the vegetable chyle produced three times as much carbon as the animal chyle, whence we may conclude that the latter contains a much greater proportion of hydrogen and nitrogen.³ It is not improbable that in this case, the vegetable chyle was less completed or assimilated, in consequence of the animal being fed upon a diet, which was not natural to its digestive organs; for we observe that the chyle of the horse, as examined by Vauquelin, which must have been derived from vegetable food, was in a more

³ *Med. Chir. Tr.* v. vi. p. 618. et seq.

animalized state than the vegetable chyle in Marcet's experiments. Dr. Prout's results, for the most part, agree with those of Vauquelin and Marcet. The chyle was found to consist of a coagulum and a fluid part, which bore a general resemblance to the corresponding ingredients of the blood. In addition to these there was the oily or fatty matter, which appears, however, to have been in less quantity than in the animal chyle which was examined by Marcet. Dr. Prout likewise compared the chyle as produced from vegetable and from animal food, and found the former to contain more water and less albuminous matter, while the fibrous part and the salts were nearly the same in both; they are both said to have exhibited a trace of the oily matter; upon the whole he found less difference between the two kinds of chyle than had been noticed by Marcet. Dr. Prout has given us an interesting account of the successive changes which the chyle experiences in its passage along the vessels, having examined it when it first enters the lacteals, when it has arrived nearly at their termination, and when it is finally deposited in the thoracic duct. Its resemblance to the blood, as might be expected, was found to be increased in each of these successive stages of its progress.*

The chyle, as it is formed or separated, is taken up

* Ann. Phil. v. xiii. p. 22. 5; see also Magendie, Phys. t. ii. p. 154. 8. Magendie observes, that the opaque white matter, which is observed in the serous part of chyle, is more abundant when the animal has used any considerable proportion of fat or oil in its diet; Ibid. p. 157.

by the lacteals, a set of vessels, the appropriate office of which is to convey this substance from the duodenum to the thoracic duct. Upon examining the contents of the different parts of the alimentary canal, we observe that the chyle first makes its appearance soon after the chyme leaves the pylorus, that the greatest quantity of it appears to be formed at a short distance from this part, more especially, as it is said, near the orifice of the biliary duct, and that it gradually occurs in less and less quantity, as we pass along the small intestines, until it is no longer to be met with, and that, except in certain morbid states, where the contents are propelled with undue rapidity, no chyle is ever found beyond the small intestines.

The obvious and essential use of the large intestines is to carry off from the system the refuse matter, after the separation of the chyle from it; we may, however, suspect that in this, as in all other analogous instances, some secondary purpose of utility is served by them. This opinion is farther rendered probable by their anatomical structure, for besides their length, which although less than that of the small intestines, is still considerable, there is evidently a provision in them for retaining their contents, and preventing them from passing too rapidly through them. It is moreover observed, that there is an obvious change in the physical properties of the contents of the intestines from the time when they enter the cæcum until they arrive at the rectum. Although they no longer contain chyle, and are there-

fore not furnished with lacteals, they have a number of lymphatic vessels connected with them, which absorb the more fluid parts of the fæces, and thus extract from them what may ultimately contribute to nutrition. That this is the case is rendered probable by the effect of nutritive matter injected into the rectum, which, in cases of mechanical obstructions of the œsophagus, when food cannot be received into the stomach, has supported life for a certain length of time, proving the capacity of the organs to extract any portion of nutriment which may be mixed with their contents. Probably, however, the most important object to be gained by the structure of the large intestines, is to retain, for a certain length of time, the fæcal matter, which is gradually transmitted to them by the upper part of the canal, and to allow it to be evacuated at certain intervals only; a temporary detention of the contents being thus rendered necessary, advantage was taken of this circumstance to produce other beneficial effects in the system.³

³ Scemmering, *Corp. Hum. Fab.* t. vi. p. 334. . 8. § 241. In the paper of Dr. Prout's, to which I have referred above, we have a series of very interesting observations on the successive changes which the alimentary mass experiences in its progress along the intestinal canal, both in different animals, and in the same kind of animal, when fed upon different kinds of food. It appears, as a general principle, that the process of digestion is more complete when animal food is employed, but we find that, in most cases, the fluids of the intestines continue to coagulate milk, even as low down as the rectum; *Ann. Phil.* v. xiii. p. 115 . . 22. I have already offered some observations upon the hypothesis of Sir Ev. Home, in p. 353.

There are two of the abdominal viscera, which, from their connexion with the stomach, have been generally supposed to be subservient to the process of digestion; the pancreas and the spleen. The former of these, both from its intimate structure, and from the nature of the secretion which it furnishes, appears to be very similar to the salivary glands, and its office has accordingly been supposed to be that of providing a quantity of fluid resembling the saliva, which may contribute to the completion of the process of chylication.⁶

The structure and function of the spleen is more obscure, and they have given rise to many hypotheses and conjectures which appear to be altogether unfounded, wholly unsupported either by any well ascertained facts, or by the analogies of the animal economy.⁷ We are indebted to Sir E. Home, for what he conceives to be a more consistent account of the nature and use of this organ. He supposes that the spleen serves as a reservoir or receptacle for any fluid that is received into the stomach, more than what is sufficient for the purposes of digestion; that this excess of fluid is not carried off by the intestines, but is transmitted directly to the spleen by the communicating vessels, and is lodged there until it is gradually removed, partly by the veins and partly by the absorbents. He

⁶ See Santorini's fig. 1, in tab. 13. also references in p. 343.

⁷ See Haller, *El. Phys.* lib. xxi.; also Sæmmering, t. vi. p. 149. et seq. where the various uses that have been assigned to the spleen by physiologists are enumerated; the author does not offer any opinion of his own upon the subject.

illustrated his opinion by numerous experiments upon living animals, in which coloured infusions were injected into the stomach, and were afterwards discovered in the spleen, while it appeared that they had not passed through the absorbents of the stomach.⁹

Sir E. Home has more recently investigated the structure of the spleen, and is led to conclude, that it consists entirely of a congeries of blood-vessels and absorbents; he conceives that there is no cellular membrane interposed between them, but that there are interstices which are filled with blood that exudes through certain lateral orifices in the veins, which are rendered pervious when these vessels are much distended.⁹ Notwithstanding the importance which we must attach to these observations, we may remark concerning them, that as the investigation of the intimate structure of the spleen appears to have been effected merely by successive macerations, it may be questioned how far this operation was the best adapted for elucidating the natural state of the organ. The conclusion which is drawn respecting the function of the spleen, is nearly similar to the one referred to above, although, perhaps, rather more vaguely expressed. It is said that, "the spleen, from this mechanism, appears to be a reservoir for the superabundant serum, lymph, globules, soluble mucus, and colouring matter, carried into the circulation immediately after the process of digestion is completed."

⁹ Phil. Trans. for 1808, p. 45. et seq. and p. 183. et seq.

¹⁰ Phil. Trans. for 1821, p. 35. 42; pl. 3. 8.

140 We have a still later account of the structure and functions of the spleen by the Professors Tiedemann and Gmelin, derived from a series of experiments which they performed on this organ, and which tend still farther to elucidate this intricate subject. With respect to the former of these points, the authors conceive, that the structure of the spleen essentially resembles that of the lymphatic glands, and that it is in fact to be regarded as an appendage to the absorbent system. Its specific function is to secrete from the blood a fluid, which is of a reddish colour, and possesses the property of coagulating, and which is carried to the thoracic ducts, and being there united with the chyle, converts it into blood. Their opinion is founded upon the appearance of this peculiar fluid, which has been seen by other physiologists as well as themselves, and which has led to an opinion, that has been pretty generally adopted, that the spleen essentially contributes, in some way or other, to the process of sanguification. They also adduce in favour of their doctrine the relation which the vessels of the spleen bear to each other. The artery is unusually large, so as to render it probable, that it must serve some purpose besides the mere nutrition of the part, while we have, at the same time, an extraordinary number of lymphatic vessels. Hence it is inferred, that the great quantity of arterial blood which is sent to the spleen, must have some immediate connexion with the numerous lymphatics that pass off from it; and they accordingly found, agreeably to the observations of preceding anatomists, that injections of

various kinds are readily transmitted from the branches of the splenic artery to the lymphatic vessels, showing an actual communication between the arterial and absorbent systems.¹ There is undoubtedly much valuable information contained in the researches of the Professors Tiedemann and Gmelin, and it is impossible not to admit that their opinions evince an accurate acquaintance with the operations of the animal œconomy. But their hypothesis appears to me to be obnoxious to one fatal objection, that animals have been known to live for an indefinite length of time, after the removal of the spleen, without any obvious injury to any of their functions,² which could not have been the case, if the spleen had been essentially necessary for so important an operation as that of chylicification. It would seem from this circumstance, that the office of the spleen must be something of a supplementary or vicareous nature only, which, although occasionally useful, is not at all times essentially necessary to our existence.

It has been made a subject of inquiry, whether by the conversion of aliment into chyle it is rendered soluble in water, because it has been supposed that

¹ Ed. Med. Journ. v. xviii. p. 285. et seq. I must acknowledge my obligations to the editors of this work, for the valuable analyses which it frequently contains of the labours of the German physiologists.

² Baillie remarks that the spleen is occasionally wanting, and has been removed without apparent injury; *Morb. Anat.* p. 260, 1; *Works by Wardrop*, v. ii. p. 235. I consider it quite superfluous to adduce any additional proofs of any fact which is sanctioned by so high an authority.

no substance could enter the lacteals unless in a state of complete solution. Arguing from the chemical nature of chyle, as examined out of the body, we must conclude that it is not absolutely soluble, although reduced to that state of minute division, which renders it easily diffusible through water, and capable of forming with it a uniform emulsion. It is not, however, improbable, that the chyle may resemble blood in its relation to water, and that it may be soluble in this fluid while in the vessels, although only partially so when removed from them.

Another subject of inquiry has been, whether any part of the food which is received into the stomach, is taken up by the absorbent vessels unchanged, without having undergone decomposition, and entered into new combinations. To this I feel disposed to reply in the negative. It is obvious that it must be the case with vegetable food of all descriptions; and with respect to the animal food employed in diet, the specific properties of all the substances appear to be so entirely altered, as to indicate that none of them had escaped the action of the gastric juice. And indeed, were any thing to pass into the duodenum unchanged, we might conclude, that the power which the lacteals appear to possess of absorbing those substances only which are perfectly assimilated, would prevent them from taking up whatever had escaped previous decomposition.

But although we conceive that every thing which can be considered as properly alimentary, must be completely decomposed, and enter into new chemical

relations before it can be converted into chyle, it appears that there are certain substances that are mixed with the food, or make a part of it, which pass into the system without experiencing any change; or even if they be in some respects changed, they are not assimilated with the chyle. This, we may presume, is the case with the saline substances that are taken into the stomach; and it appears that there are certain bodies which give the specific odours and flavours to various articles, both of animal and vegetable origin, which are taken up by the lacteals, while they still retain their sensible properties, and impart them to the solids and fluids of the animal. It is well known that the specific qualities of various medicines are communicated to the milk, so that the milk will affect the child in the same manner, as if the substances had been directly taken into the stomach; and every one is aware that the flavour of the milk, and of the flesh of animals generally is materially influenced by the nature of their food. The sensible properties of the plants on which bees feed, are imparted to their honey; and we are informed by naturalists that it sometimes becomes poisonous, when procured from noxious vegetables, and the same is said to have been the case with the flesh of birds, in consequence of their feeding upon berries or seeds, which, although salutary to them, are injurious to the human constitution. Besides the substances which pass into the circulation unchanged, there are certain bodies, which, although not completely assimilated with the chyle, yet seem to be partially

decomposed, so as to acquire new sensible properties which they impart to the solids or fluids of the animal. Turpentine presents us with a remarkable example of this kind, which, when taken into the stomach, imparts to the urine an odour exactly resembling that of violets.³

In the last chapter I offered some remarks upon the various salts which are found in the blood, and I discussed the question whether we are able to account for the quantity which exists in the different solids and fluids, by supposing them to be introduced along with the food. The facts of which we are in possession, would lead us to the conclusion, that the quantity of these earths or salts that are introduced into the stomach *ab extra*, is not sufficient to supply the demands of the system, but that they must, in some way or other, be generated by the vital powers. Our next inquiry will be, by what powers, or in what part of the system, are they produced; they may either be formed by the digestive organs, during the process of chymification or chylicification, or by a process more analogous to secretion, by an organ or organs expressly appropriated to the purpose. The difficulties which

³ Fordyce states that indigo and musk are both taken up by the lacteals, so far retaining their previous properties, as that the former possesses its specific colour, and the latter its peculiar odour; *On Digest.* p. 122. This power is, however, limited to certain substances, while others, although equally exposed to the action of the lacteals, are incapable of entering them, these vessels thus exercising what appears to be a kind of selection; p. 123; but upon this subject I shall have occasion to offer some further observation in the next chapter.

exist upon both these suppositions are very great; we can indeed form no conception of the nature of the operation in either case; and it is only in consequence of the necessity that there is of attempting some explanation, that we have recourse to them. The subject is again introduced into this place for the purpose of inquiring whether there be any grounds of preference between these two opinions; and we may go so far as to remark, that if we find in the chyle all the salts that exist, in any of the constituents of the body, we must conclude that they are produced in the process of digestion, and afterwards merely separated from the blood; a conclusion to which the results of our experiments would seem to lead us. The whole subject is, however, one that is so extremely obscure, and of such difficult solution, that I do not think it desirable to enter more particularly into the consideration of it, until we are in possession of a more firm basis of facts, on which to build our hypotheses.

§ 4. *Theory of Digestion.*

There are few subjects in physiology that have afforded a more fruitful subject for speculation than the theory of digestion, for in this, as in other parts of the science, we may remark, that the more obscure is the subject, and the less real information we possess concerning it, the more numerous have been the attempts to frame hypotheses to account for it. The opinion proposed by Hippocrates, and adopted by Galen and the ancients generally was, that the aliment is digested by what is termed concoction. But

this is to be considered rather as another word expressive of the action, than as any explanation of it. It is, indeed, synonymous with the term digestion, and derived from the same analogy of the change which substances undergo, when they are exposed to a certain degree of temperature in close vessels.*

The next hypothesis was that of putrefaction, an hypothesis which was maintained by some of the earlier chemists, and was supported by various observations, and even experiments, that were supposed to be favourable to it. The food, when it is received into the stomach, was observed to have its texture broken down, and to have acquired an unpleasant odour, which the older physiologists, according to the loose mode of reasoning which they employed, regarded as a species of putrefaction. It is a sufficient refutation of this hypothesis to remark, that digestion and putrefaction are processes of a totally different nature;

* See Boerhaave, *Prælect. not. ad.* § 86. t. i. p. 158, 160; Blumenbach, *Inst. Phys.* § 360, p. 202. By the following passage in Celsus, it appears that the hypothesis of attrition and of putrefaction, had also their defenders among the ancients: "Duce, alii, Erasistrato, atteri cibum in ventre contendunt: alii, Plistonico Praxagoræ discipulo, putrescere: alii credunt Hippocrati, per calorem cibus concoqui." *Præf.* p. 6. § 10. The student, who is disposed to make himself acquainted with the doctrines of the earlier physiologists on the subject of concoction, a process which was supposed to be concerned in other functions besides that of digestion, may consult the treatise of Fernel, *Physiol. lib. 6. c. 6. "de concoctionibus."* Fernel was one of the first of the moderns who wrote from his own observations, and who exercised his own judgment on subjects of physiology and pathology.

and that so far from their having any connexion with each other, one of the first effects of the gastric juice is to resist putrefaction, or even to suspend it, if it has actually commenced.⁵

The mechanical physiologists endeavoured to account for all the phenomena of digestion by trituration, and they performed many curious experiments in proof of their opinion on those animals which are said to possess muscular stomachs. But although the facts were correctly stated, the conclusions which were deduced from them were inaccurate in two respects. In the first place, they extended to all classes of animals an action which belongs to certain species only, and secondly, in considering the trituration of the muscular stomach as analogous to the process of chymification in membranous stomachs. I have already stated, that the operation of the gizzard is entirely mechanical, and is equivalent to the teeth of

⁵ This hypothesis has had its advocates even in modern times; Cheselden, *Anat.* p. 155, says "digestion is no other than corruption or putrefaction of our food." It would appear to have been invented by Plistonius, of whom nothing more is known than that he was the author of this hypothesis; see Celsus, *ut supra*, and Le Clerc, *Hist. de la Med.* part. 2. liv. 1. c. 8. p. 326, 7. The hypotheses of Pringle and M'Bride, although nominally founded upon fermentation, ought really to be referred to putrefaction. M'Bride and most of his contemporaries thought that the saliva was an active promoter of this decomposition, *Essays*, p. 16, 7: whereas Pringle's experiments; *Appendix*, p. 362; led him to conclude that saliva resists this process. With respect to the gastric juice, the fact appears to be that it is decidedly antiseptic; Stevens, c. 9; Spallanzani, *Exper.* § 249, 259.

quadrupeds; the food is then brought into the same state as it is by mastication, and has still to undergo the action of the proper digesting stomach. If direct facts were wanting to confirm this opinion, they are abundantly furnished by the experiments of Stevens and Spallanzani, as they prove that chymification is effected under circumstances in which trituration could not possibly operate.⁶

In opposition to the mechanical doctrine of trituration, an opinion was advanced by the earlier chemists, that the action of the stomach consisted in a species of fermentation. This hypothesis appears to

⁶ Physiological speculation was, perhaps, never carried to a greater excess than by Pitcairn, in the estimate which he makes of the mechanical force which the stomach exercises in digestion. After employing much learned and abstruse discussion to prove that no other power is competent to produce the requisite effect upon the aliment, he calculates that the power of the muscular fibres of the stomach is equal to 12,951 lbs.; *Dissert.* p. 72. 95; also *Elem. c. 5. p. 25. 7*; see the observations of Cheselden, *Anat. p. 152. 5*; also of Hales, who estimates "that 20 lbs. would come nearer to the pressure of the aliments of a full stomach;" *Statistical Essays, v. ii. p. 174, 5*.

Haller very explicitly states the impossibility of trituration being effected by a membranous stomach; *El. Phys. xix. 5. 1*; yet he scarcely draws a sufficiently accurate line of distinction between the mechanical and chemical action of this organ. It is amusing to observe the learned and laboured arguments which Fordyce thought it necessary to adduce in order to prove that minute mechanical division alone cannot alter the chemical nature of a substance; *On Digest. p. 124. 138*. See remarks on trituration by Stevens, *De Alim. Concoct. cap. 10*; also by Richerand, *El. Phys. § 18. p. 100. 2*.

have been originally suggested by Vanhelmont,⁷ and was, at one period, embraced by the most celebrated physiologists of the age.⁸ In order to estimate the

⁷ The account which this singular writer gives of the action of the stomach upon the aliment, is contained in his treatise entitled, "*Sextuplex Digestio Alimenti Humani.*" According to his doctrine, all the changes which the components of the body experience, in the different abdominal viscera, are to be ascribed to a series of fermentations; of these there are supposed to be six, the first being the conversion of aliment by the stomach, "*in cremorem, plane diaphonum in cavo stomachi.*" He goes on to observe, "*Ad id, id fieri vi fermenti primi, manifesti a liene mutuati.*" Vanhelmont, although a man of considerable acuteness and information, partook largely of the arrogance which is so characteristic of his sect. The first paragraph of the above treatise is entitled, "*Misera Galenicorum jactura,*" (a censure which would apply with far more justice to the chemists) and he begins by accusing Galen himself of having described parts which he never saw, and of writing without any knowledge, or any attempt to investigate the truth; *Ortus Med.* p. 167.

⁸ Among the physiologists of eminence, during the last and preceding centuries, who have attributed the digestive process to fermentation, I may mention the names of Sylvius, *Dissert. Med.* 1. et *prax. Med. lib.* 7. C. 7; Willis, *De Ferment.* C. 1. p. 17; Boyle, *Works*, v. ii, p. 622; Grew, *Comp. Anat. &c.* p. 26; Charleton, *Œcon. Anim. Exerc.* 2. de chylif.; and Lower, *De Cord.* p. 204. Boerhaave's opinion was, that a commencement only of fermentation is excited by the gastric juice, *Prælect.* t. i. § 78, 87; and the same appears to be the case with Haller, *Prim. Lin. Sect.* 24. and *El. Phys.* xix. 5. 2. Parr inclines to the same doctrine; *Dict. "Digestion."* The objections to the doctrine, according to the opinion that was entertained at the time respecting the nature of fermentation, are well stated by Stevens, *De Alim. concoct.* C. II. Spallanzani devotes his sixth dissertation to the inquiry, whether fermentation is a necessary part of the process of digestion, which he

value of their hypothesis, we must bear in mind, that they employed the term fermentation to express any change which a body experiences in its mechanical or chemical properties, either by the action of its constituents upon each other, or by the addition of a foreign substance, in consequence of which the elements of the body were made to enter into new combinations. Various species of fermentations were supposed to exist, and a number of the most important changes, especially of those that occur among organized bodies, were supposed to be produced by this kind of operation. The correctness of this hypothesis must depend very much upon the exact sense in which the term is employed, for it must be admitted, that according to the mode in which it was used by the older writers, the change produced upon the aliment by the stomach, appears to fulfil all the conditions that were supposed to be requisite to fermentation.

The hypothesis of chemical solution, which is considerably analogous to that of fermentation, was derived from the experiments which have been so frequently referred to, on the effect of the gastric juice upon the aliment taken into the stomach, which was supposed to be similar to that of a chemical solvent.⁹

decides in the negative; he, however, conceives that the absence of any gaseous product is a sufficient proof of the non-existence of fermentation, as Pringle and M'Bride thought that its presence was a sufficient indication of that process.

⁹ It may not be uninteresting to observe the manner in which Grew's opinion on this subject corresponds with the modern doctrines; he published his lectures in 1681. "By the joint assistance of the glandulous and the nervous membranes, the

In describing the successive changes which the food undergoes after it is received into the stomach, I have had occasion to remark upon the facts that have been adduced in favour of this doctrine, and upon the objections that have been urged against it. We appear to have sufficient evidence to prove that the stomach secretes a peculiar fluid, which acts chemically upon the aliment, and that nothing farther is necessary to produce this action than to bring the substances into contact.¹ That this is a case of mere chemical action, is especially proved by the experiments of Stevens,² and still more of Spallanzani, where he produced a similar change out of the body, by gastric juice procured from the stomach,³ and also by the action of rennet upon milk. To a certain extent

business of chylication seems to be performed. The mucous excrement of the blood being supplied by the former, as an animal corrosive preparing; and the excrement of the nerves by the latter, as an animal ferment, perfecting the work;" Ubi supra, p. 26.

¹ Reaumur, in two papers in *Mem. Acad. pour 1752*, p. 266. et seq. and p. 461. et seq. contrasts the process of digestion in birds with muscular and with membranous stomachs, in the first of which the great agent is supposed to be of a mechanical, and in the second of a chemical nature. He had considerable merit in pointing out this distinction, but he was not fully aware of the difference of mere trituration and of proper digestion. Reaumur's experiments were repeated and much extended by Spallanzani. See also Blumenbach, *Inst. Physiol.* § 358, 9; Monro *Tert. Elem.* v. i. p. 532.

² *De Alim. Concoct.* § 24, 5.

³ *Experiences Dissert.* 2. § 85. et seq. *Dissert.* 4. § 149, 0. 186. *Dissert.* 5. § 216 et alibi.

these experiments may be considered as establishing the fact; yet the hypothesis is still encumbered with serious difficulties. Not to insist upon the objection, that it only explains one part of the process, the formation of chyme, it must be confessed, as I have remarked above, that it is not easy to conceive how all the various articles that are taken into the stomach can be converted into a substance of the same, or nearly the same consistence, and this by an agent apparently so little active as the gastric juice.

In consequence of these objections, and of the difficulty which there seemed to be in accounting for digestion, either upon mechanical or chemical principles, many of the most eminent among the modern physiologists have ascribed it to the direct agency of the vital principle. It is said that the interior surface of the stomach is endowed with a specific property, unlike any other that exists in nature, which belongs to it as a living substance, and which enables it to digest the food. In proof of this position the curious fact is adduced which was observed by Hunter, that in some cases of sudden death, the stomach itself is partially digested by the gastric juice which had been previously secreted.⁴ A fact of a similar kind

⁴ Hunter's original observations are contained in his paper in the Phil. Trans. for 1772, p. 447. et seq.; they were afterwards given in an enlarged form in his *Observ. on the Anim. Econ.* p. 226. 1; see *Baillie's Morb. Anat.* Ch. 7. p. 148, 9; *Works by Wardrop*, v. ii. p. 136, 7. Eng. to *Morb. Anat.* fasc. 3. pl. 7 fig. 2. In the 1st vol. of the *Trans. of the Edinburgh Med. and Chir. Soc.* p. 311. et seq. we have a very valuable paper by Dr Gardner, on erosions and perforations of the alimentary

is stated with regard to certain species of vermes, that are occasionally found in the stomachs of animals, and which, as long as they remain alive, are not acted upon by the gastric juice, although after death it affects them as it does any other organized substance.

These facts are very curious and important, and they clearly prove that there is a difference in the mechanical and chemical relations of living and dead matter, a difference which, I fully admit, we are not able to explain or account for. It is the same kind of difficulty which occurs with regard to the contractile and sensitive functions of the muscles and the nerves, that they are both of them totally destroyed by the extinction of life, although for some time afterwards, neither of these organs seem to have undergone any alteration, either in their chemical or physical properties. In this, as in the other analogous cases, the doctrine of the animists proceeds upon the principle, that no modification of the laws of chemistry or mechanics can account for the phenomena, and that it is consequently necessary to assume the existence of some new agent to meet the emergency. But I may remark, as I have done on former occasions, that by this proceeding we throw no new light upon the difficulty, and that in reality, we are only employing
canal, in which the author gives us an account of some cases that had fallen under his own observation, as well as a collection of the observations of others. In Beck's Med. Juris. by Dunlop, p. 376. . 380. we have many references and good remarks.

a different expression to announce the fact, and one which is less simple and intelligible. With respect to the particular case under consideration, our knowledge of the process is, in many respects, extremely incomplete; we have only an imperfect acquaintance with the successive steps of the operation, and we are, in a great measure, ignorant of the nature of the agents by which it is effected; nothing therefore can be more incorrect than thus prematurely to attempt to establish general principles before we are thoroughly acquainted with the facts on which they profess to be founded.

This is essentially the hypothesis which is maintained by Fordyce in his elaborate treatise. He remarks that aliment, when it is converted into chyle, undergoes a complete change in its elementary constitution, and he admits that the gastric juice is a chemical agent, but he contends that the nature of the action is totally unlike what takes place in any other chemical process, and that it is therefore necessarily connected with the vitality of the stomach. The anomaly on which he particularly insists is, that by adding the same agent, the gastric juice, to various kinds of aliments, we have always the same product, as he maintains that the chyle of carnivorous is perfectly similar to that of herbivorous animals. But in order to prove the point which he wishes to establish he ought to show not only that the chyle, but that the fæces also, are similar. He further conceives that it is inconsistent with the principles of chemical action to suppose, that a single menstruum can form

with a single principle, as, for example, farina, the three bodies which constitute chyle. He likewise asserts that chyle cannot be formed out of the body, and that if aliment be placed in a dead stomach it will not undergo the same changes as in a living stomach, although they are both kept at the same temperature.⁵

Somewhat allied to the hypothesis of the animists, although much less vague and indeterminate, is the doctrine which has been lately advanced, that digestion is essentially a nervous function, or one that depends upon the immediate and direct agency of the nervous system. A number of well known occurrences, not only in pathology, but in the ordinary actions of the animal œconomy, prove to us that the powers of the stomach are intimately connected with the nervous system.⁶ And indeed we might conclude

⁵ On Digestion, p. 139.. 146, 171.

⁶ It is unnecessary to adduce particular examples of the connexion between the state of the nervous system generally, and the action of the digestive organs, as they must be sufficiently obvious to every one, from the result of his own experience. This, however, as I have had occasion to remark on various occasions, proves no more, than that the nerves are the media by which the different parts of the system are connected together. Grief, and the other depressing passions, act upon the circulating system, and perhaps also from other causes, the secretions are diminished, and that of the gastric juice among the rest. At the same time the nervous system becomes less sensitive, therefore the feeling of hunger is less felt, and consequently we are less disposed to take the proper quantity of nutriment. Hartley supposes that "the stomach is particularly affected in grief;" On Man, v. i. p. 189; an opinion which receives some counte-

that this would be the case, by an inspection of the anatomical structure of this organ, as it cannot be doubted that some useful purpose must be served by the variety and number of the nerves with which the stomach is provided. But there are many purposes to which the nerves may be subservient, besides the digestion of the food, while, on the other hand, it may be asserted, that we can form no idea of the mode in which the mere chemical action of two bodies can be affected by the nervous influence. The experiments, of which I have already given an account, in which the digestion was suspended by dividing the *par vagum*, although by some eminent physiologists they have been thought decisive in favour of the nervous hypothesis, yet even if we admit them in their fullest extent, they go no farther than to prove the agency of the nerves in the preparation of the gastric juice, they therefore refer entirely to the question of secretion, and as such have been already considered in the last chapter. There is indeed one point of view in which we may regard these experiments as bearing upon the subject of digestion. It may be conceived that the presence or absence of the nervous influence depends upon the peculiarity in the nerves of this organ. See also the remarks of Scemmering, in § 179. "*consensus ventriculi, cum aliis partibus in genere;*" he extends these observations to the various organs; § 180. 4. Vanhelmont's well-known opinion that the stomach is the immediate seat of the soul, or the centre to which all the affections of the nervous system are referred, although in itself so palpably absurd, is enforced by a number of ingenious and just observations on the extensive influence which this organ exercises over the whole system; *Ortus Med.* p. 248, 9. 450.

of the nervous influence is the cause of the difference which we observe between living and dead matter; that perhaps the electricity, which has been supposed to be identical with the nervous influence, may operate so as to prevent the chemical change, which, under ordinary circumstances, leads to the decomposition of organised bodies, and may likewise prevent it from being acted upon by the gastric juice. It is impossible to say that some effect of this kind may not take place; but as we have no evidence of its existence, it would be premature to assume it as the basis of our hypothesis.

From this review of the various speculations that have been offered to account for digestion, there appear to be two, which are, to a certain extent, countenanced by the phenomena, and which show some analogy between the other operations of nature and the effects which are produced by the stomach. These are the hypotheses of fermentation and of chemical solution; it remains therefore for us to consider, which of these presents us with the nearest resemblance and the closest analogy to the operations of the stomach. It is unnecessary to premise that both of these are to be referred equally to the laws of chemical affinity, and that the point to be determined therefore will be, whether the process of digestion is merely to be referred to chemical action generally, or whether it can be approximated to the particular case of fermentation.

The essential difference between the two cases may be conceived to be, that whereas in what may be

strictly termed chemical solution, we have two bodies that act upon each other, and produce a third substance, exhibiting new properties; in fermentation this change is effected by the action of the elementary parts of a body upon each other, either without any addition ab extra, or by adding a very minute quantity of an agent, which serves merely to establish the commencement of the operation, and is afterwards no longer necessary to its continuance.⁷

The term fermentation, or rather the corresponding Greek word, *Ζυμωσις*, seems to have been originally employed to express the spontaneous change which takes place in bodies, attended with an enlargement or tumefaction of their substance, in consequence of the extrication of some volatile matter or vapour. See Castelli, "fermentatio." Vanhelmont is said to have been the first among the moderns, who used the word fermentation, applying it to the change which dough experiences in forming wheaten bread. See Stahl, *Fund. Chym. Dog. Rat. et Exper.*, Pars 2. Sect. 4. § 2. At one time it was employed in a vague and general way to designate almost every chemical change to which a compound body is liable; see page 390; but it has been of late restricted nearly in the manner stated in the text. Still, however, the most correct among the modern chemists are not entirely agreed as to the nature of the processes which ought to be classed among the fermentative, and they consequently differ in the number of the species of fermentations which are supposed to exist. Stahl appears to have contributed to produce a more correct idea of the operation, by confining it to a spontaneous change among the constituents of a body, or one which was brought about without the addition of any foreign ingredient. *Ubi supra*, § 3; also *Fund. Chym. Dog. et Exp. Art. 3. Cap. 2. § 2*. He supposes that it may exist not only in vegetable and animal, but also in mineral substances. The later chemists have, however,

The species of fermentation with which we are the most familiar is the vinous, by which a solution of mucilage and sugar is converted into carbonic acid and alcohol. These substances are produced by a mutual interchange which takes place between the elements of the substances contained in the solution, but the effect is considerably promoted by the addition of a portion of yeast, which is the result of a previous fermentation, and which establishes the

restricted it entirely to vegetable and animal bodies, and some of them, as Thomson, *Chem.* v. iv. p. 370; Murray, *Chem.* v. iv. p. 391; and Brande, *Man.* v. iii. p. 128. § 1855, confine it to the former. The number of fermentations generally supposed to exist are three, the vinous, the acetous, and the putrefactive; Mr. Brande, however, limits the number to two, the vinous and the acetous, correctly, as I think, rejecting the putrefactive, this being rather a complete decomposition of the body, than a conversion of it into new definite products. Thenard, *Traité*, t. iii. p. 471, enumerates four, adding the saccharine to the three former, and some chemists have also admitted the panary, or the fermentation of dough; Aikin's *Dict.* "Bread;" I conceive that there is a foundation for both the saccharine and the panary. Thenard supposes that the panary is merely a compound of the vinous and the acetous, depending upon the presence of farina and sugar in the flour. Vogel has, however, shown that after all the sugar is carefully removed, wheaten flour is still capable of fermenting; *Journ. Pharm.* t. iii. p. 214; and Vauquelin remarks that there is no sugar in the potatoe; *ibid.* p. 320. We have certainly no evidence of the existence of alcohol in fermented bread, and it is equally certain that the most perfectly fermented bread is not necessarily acid. Dumas enumerates no less than six fermentations, the spirituous, the acid, the putrid, the muriatic or saline, the saccharine, the panary, and the "colorante," which develops the principle of colours; *Physiol.* t. i. p. 306, 7.

commencement of the process, and causes it to proceed with more rapidity. To which then of these cases is the action of the stomach more analogous, to simple solution, or to fermentation? Does the gastric juice act more like a solvent or a ferment? I have already stated the objections which have been urged against the idea of its acting as a solvent; and it has been objected to the hypothesis of fermentation, that the products of the action of the stomach do not resemble those of the vinous fermentation, or of any other with which we are acquainted. But this is no objection to the hypothesis itself, as we have no reason to conclude that there may not be other operations, that are strictly entitled to the name of fermentation, besides those that are generally recognised by chemists. Besides the vinous and the acetous, we have the panary fermentation, that takes place in dough, which seems certainly entitled to the appellation, and there is no reason to conclude that others may not exist.⁸ There are several circumstances in which I conceive that digestion bears

⁸ Sir E. Home informs us, on the authority of Sir H. Davy and Mr. Brande, that an inflammable gas is extricated in the third stomach of ruminant animals; *Phil. Trans.* for 1807, p. 163; and there is reason to believe, that the gas which is evolved during digestion, generally contains more or less of hydrogen. Sir E. Home observes, that this circumstance establishes a difference between digestion and fermentation; it, however, only shows that the fermentation of the stomach differs from that of alcohol or acetic acid. See the analyses of the gases in the different parts of the human alimentary canal by Jurine and Chevreul; p. 490.

an analogy to fermentation, and in which it appears rather to resemble this operation, than what may, with greater strictness, be stiled chemical solution.

1. The substances possessed of various properties, and composed of elements combined in different proportions, are all reduced to an homogeneous mass, by the addition of a minute quantity of an extraneous body. 2. The quantity of the ferment, the substance which is the immediate agent in this process, is often extremely minute in proportion to the effect which it ultimately produces. 3. The subjects of fermentation are the products of organization alone. 4. The process is frequently deranged or suspended by apparently very slight causes, and such as would have previously appeared altogether inadequate to the effect. 5. When the process is completed, if the substances are kept in the same situation as at first, a new operation sometimes commences, inducing a new fermentation, which terminates in the production of a new substance, possessed of peculiar and specific properties different from the one originally produced. The analogy might perhaps be extended; but I conceive that enough has been stated to prove that it exists, and to show that in some remarkable and even essential particulars, the process of digestion resembles that of fermentation.⁹

⁹ The experiments of Pringle, *Observations*, Appendix, p. 346, 364, et alibi; and still more those of M'Bride, *Essays*, N^o. 1; were supposed, at one time, to be almost decisive in favour of the doctrine of fermentation. But it may be remarked concerning them, that they are little applicable to what takes

I must, however, remark, that even if we were able fully to establish this point, it would only serve to explain one step in the operation, the conversion of aliment into chyme; for in the subsequent change of chyme into chyle, we appear to have neither the requisites for fermentation, nor have we any of those phenomena which characterise chemical action. It is, however, perhaps equally difficult to explain this change upon the principle of mere chemical solution, as we seem to be altogether at a loss to determine by what agency the chyle is formed, or separated from the mass with which it is combined. But this point appears to be altogether so completely involved in obscurity, that it will probably be desirable not to attempt any explanation of it, until we have acquired a more correct knowledge of the nature of the phenomena themselves.

In the mean time, I shall point out some circumstances which it will be proper to attend to in any future investigations, or experiments, that may be

place in the stomach, and merely show us in what degree different alimentary matters are liable to spontaneous decomposition, when placed under certain circumstances. The opinion which they both of them entertained on the subject of fermentation was not sufficiently correct; Pringle made no distinction between fermentation and the entire destruction of a body, and M'Bride's account of it, although more precise, is still defective. He defines it, "an intestine motion, which arising spontaneously among the insensible parts of a body, produceth a new disposition, and a different combination of those parts;" p. 2. In this definition, as well as in Pringle's, the results are scarcely taken into consideration.

made upon this subject. And, in the first place, I conceive it will be desirable to examine with more minuteness than has hitherto been done, the changes which the gastric juice produces upon alimentary matter out of the body; for, although we have every reason to be satisfied with the diligence and accuracy of Spallanzani, yet so many improvements have taken place in the method of performing experiments, and of making observations, since his time, that it is not unfair to expect that some important information might be gained by a repetition of them. It would considerably contribute to our knowledge upon the subject, if we were able decidedly to ascertain whether chyle is contained in chyme, and, consequently, that the office of the duodenum is only to separate it from the mass; or whether some chemical change takes place, by which it is actually produced in this organ. Could we prove that the former is the case, it would, no doubt, render the subject less complicated; although still we might be unable to show by what agent, or by what kind of process, the separation is effected. Many other topics for consideration might be suggested, but the above are of essential importance to our forming any correct notions upon the subject; until these points have been ascertained, we have no right to complain of the intricacy, or obscurity of the subject, and to speak of the process of chylification as of something mysterious, which cannot be accounted for without attributing to matter new properties, or calling in the aid of new powers to explain the phenomena.

§ 5. *Peculiar Affections of the Stomach.*

There are certain affections of the stomach, besides those which are immediately connected with digestion, which it will be necessary for us to examine; both with respect to the mode in which they are produced, and their ultimate effects upon the economy. Among the most important of these are the sensations of hunger and of nausea, and, as connected with the latter, the act of vomiting.

Hunger is a peculiar perception experienced in the stomach, depending upon a deficiency of food; which, although it has been vaguely classed among the impressions that belong to the sense of touch, is essentially different from it, and entirely of a specific nature. The efficient cause of hunger has been frequently discussed by physiologists, and it has been generally referred either to a mechanical, or to a chemical cause, according to the respective tenets of the authors. The mechanical physiologists ascribed it to the friction of the sides of the stomach, or of the folds and projections of its inner coat against each other; an hypothesis which is disproved by the anatomy of the organ, from which we learn that this kind of friction cannot exist; as from the rounded form of the stomach, as well as from its structure and composition, it would seem impossible that its different internal parts can come into forcible contact with each other.¹ Besides, the feeling of hunger is ob-

¹ Haller, however, adopts this hypothesis respecting the proximate cause of hunger, *Prim. Lin.* § 638; *El. Phys.* xix. 2.

viously of a specific nature, and totally different from the mere sense of resistance, no more resembling that which arises from the pressure of a hard body upon the skin, than the sense of sight does that of pressure upon the eyeball. The chemical physiologists, on the one hand, accounted for the sensation of hunger by the action of the gastric juice, which, in consequence of its powerful effect upon organized matter, was supposed to have a tendency to corrode the internal membranes of the stomach. But this opinion may be considered as entirely disproved by the fact, which was stated above, that the solvent power of the gastric juice is confined to dead animal matter, and is therefore incapable of acting upon the living stomach, while, at the same time, we have no reason to suppose that it possesses any corrosive properties, similar to those of a chemical acid, or which could be supposed likely to produce any painful impression upon the nerves of the part.

12; he thinks the attrition takes place between the ridges of the nervous coat; and illustrates the supposed effect by the acute pain which is experienced when friction is exercised upon any exposed part of the skin. The effects produced by long continued fasting are described with his usual minuteness; Sect. 2. § 3. . 7. Scemmering ascribes the pain from long continued fasting to the action of the gastric juice; but it does not appear whether he attributes the ordinary sensations of hunger to this cause; Corp. Hum. fab. t. vi. p. 237. § 152. See also Boerhaave, Prælect. § 88. cum not. t. i. p. 171, et seq. We have some good observations upon the phenomena of hunger, by M. Magendie; Physiol. t. ii. p. 24. et seq.; he refers the proximate cause of hunger to the action of the nervous system; p. 30. See art. "Digestion," in Dict. de Scien. Med. p. 370. . 5.

I conceive that the only explanation we can offer of the phenomena, is to consider hunger as a specific sensation produced upon the nerves connected with the stomach, in the same manner as the organs of sense have their appropriate nerves, each of them adapted to the peculiar perceptions of the organ.² It is not improbable that the action upon the nerves may be, in some way, effected through the intervention of the gastric juice, perhaps analogous to the action of light upon the retina; but this subject will be considered more fully in the part of the work which treats expressly upon the nervous system.

The sensation of thirst, although not referred to the stomach, may be noticed in this place, in consequence of its connexion with the digestive organs generally, and more particularly with the state of the stomach. It is seated in the tongue and fauces; and, in the natural and healthy state of the functions, depends upon the deficiency of the mucous secretions of these parts.³ Although it appears to possess less of a specific character than the sense of hunger, yet it probably ought to be regarded as something more than the mere sensation which would be produced by the mechanical condition of the part, and as a peculiar action on a certain set of nerves, resulting from the effect of an appropriate stimulus. But,

² See the remarks of Sœmmering, t. vi. p. 233. § 149, entitled "*Propria ventriculo sentiendi facultas*;" and § 156. "*Fames et sitis proprii sensus non sunt.*"

³ Haller, *Prim. lin.* § 639; *El. Phys.* xix. 2. 9; Magendie, *Physiol.* t. ii. p. 31..3.

although there is much obscurity concerning the efficient cause of hunger and thirst, their final cause is sufficiently obvious ; they are the means by which we are warned of the necessity for supplying the system with the materials requisite for its existence. They belong to that class of actions which are termed appetites ; where an effect, which is a compound of a physical and a mental operation, is connected with an evident useful purpose in the animal economy, and which is brought about through the intervention of the nervous system.

A variety of circumstances, differing very much in their nature and operation, agree in producing a peculiar sensation, which we refer to the region of the stomach, termed nausea. It is accompanied by a general disturbance of the different functions of the body, as well as a diminution of the powers of the muscular and nervous systems ; and if it be continued for any length of time, it produces an effort to vomit. The act of vomiting consists in an inversion of the peristaltic motion of the stomach, beginning at the pylorus, and proceeding to the cardia, by which the contents of the organ are carried back into the œsophagus, and finally rejected from the mouth. Although the action commences in the muscular fibres of the stomach itself, it is promoted by the co-operation of the muscles of the abdomen, and the diaphragm, which, indeed, contribute very considerably to the ultimate mechanical effect.⁴

⁴ The question has been much discussed, how far the muscles of the abdomen and diaphragm co-operate with the stomach itself in the mechanical act of vomiting. The opinion generally

The immediate causes of vomiting may be reduced to three classes. 1. Any substance irritating the stomach itself, such as undigested food, certain stimulating medicaments, which, from their specific action, have obtained the name of emetics, and various

adopted by the modern physiologists is, that it originates in the stomach itself; but that, perhaps in every instance, and certainly in all violent efforts, the neighbouring muscles assist the muscular fibres of the stomach. Haller, *El. Phys.* xix. 4. 12, 14. resting on the authority of Wepfer, *De Cicut. Aquat.* p. 112. Lieutaud, *Mem. Acad. pour 1752.* p. 223; et seq. Sauvages, *Nos. Meth.* t. ii. p. 337, and others, supposes that the stomach alone is competent to the operation; whereas it was maintained by Chirac and Duverney, *Mis. Curios.* Dec. ii. ant. 4. obs. 125. p. 247, 8; and *Mem. Acad. pour 1700, histoire,* p. 27, that the stomach is entirely passive. Hunter also maintained the same opinion, at least that the contraction of the muscular fibres is not essential to the act of vomiting; *Anim. Econ.* p. 199, 0; and a series of experiments has been lately brought forward by M. Magendie, in support of the same opinion. He even goes so far as to state, that when the stomach was removed, and a bladder substituted in its place, vomiting occurred, which must necessarily have been effected by the sole action of the diaphragm, and the abdominal muscles; *Mem. sur le vomissement,* p. 19, 0; and *Physiol.* t. ii. p. 138.. 0. I apprehend, however, that the ordinary opinion is the correct one, that the action commences in the stomach, but is very materially promoted by the parts external to it. The functions of the uterus, bladder, and intestines, are all favourable to this opinion; in each of them contraction evidently begins in the organ itself. The effect of dividing the par vagum has been adduced by both parties, in support of their respective opinions: the fact appears to be, that when this nerve is divided, although nausea ensues, actual vomiting does not take place. As it is to the stomach that the par vagum is especially destined, it affords a presumption that this is the part where the act commences. We have some judicious observations on the subject by Bell, *Anat.* v. iv. p. 51, et seq.

chemical acrids, which appear to produce vomiting, rather in consequence of the violent irritation which they cause, to whatever part they are applied, than of any specific effect upon the organ itself. 2. Certain irritations applied to various parts of the body, more or less remote from the stomach, but connected with it, either by the intervention of nerves, or in some way which we cannot satisfactorily explain, although we constantly recognize their operation. Among these may be enumerated certain affections of the brain, the motion of a vessel at sea,⁵ certain visible impressions upon the retina, peculiar flavours and odours, certain medical agents, when applied to other parts of the body, as to the fauces, the rectum, or even to the external surface, calculi in the kidneys, and hernia of any part of the intestinal canal. 3. Mental impressions of various kinds, depending altogether, or in a great degree, upon association.⁶

⁵ Darwin refers sea-sickness to an association with some affections of the organs of vision, which, in the first instance, produce vertigo; *Zoonom.* v. i. Sect. 20. But it may be objected that sea-sickness is not necessarily preceded by vertigo, and that blind persons are equally subject to it. Dr. Wollaston has explained the affection by a change in the distribution of the blood; the descending motion of the vessel tending to cause an accumulation of blood of the brain; *Phil. Trans.* for 1810.

⁶ Haller, *Prim. Lin.* § 653; *El. Phys.* xix. 4. 13; *Scæmmering, Corp. Hum. fab. t. vi. p. 269. . 273. § 178.* We have a detail of the opinions of the earlier physiologists in M. Magendie's memoir; but it is less complete than that given by Haller, § 14.

CHAPTER XI.

OF ABSORPTION.

WE now arrive at the last of the three functions, which I classed together, as furnishing the materials for the direct support of the system, that of absorption; the process by which the substances that serve for the growth and support of the body are carried into the blood, and are assimilated to this fluid. Although this may appear to be the primary, and, as we presume, the most essential of the effects that are produced by the process, there is also a further object which is brought about by the absorbent system. I have had occasion to remark, in various parts of this treatise, that it appears to be a general principle in the animal economy, that all the particles of which the body is composed, after a certain period, lose the power of performing their appropriate functions, and that it consequently becomes necessary to have them replaced by new matter. It is by means of absorption that this exchange of particles takes place, the former constituents being taken up by the vessels, and returned into the general circulation, to be either discharged or employed under some new form, while a different set of absorbents receive the recent matter from the products of digestion, and likewise convey it to the blood, whence it is distributed to all parts of the body. The subjects which will

more particularly occupy our attention in this chapter, are: 1. An account of the apparatus by which the process of absorption is effected; 2. The uses of the absorbent system; in the 3d place we must consider the mode in which the absorbents act, so as to receive and convey their contents; and, in the last place, we must inquire into the nature of the connexion which subsists between absorption and the other functions of the animal economy.

§ 1. *Description of the Absorbent System.*

The absorbent system, by which I mean to designate those organs which are exclusively employed in the performance of this function, may be regarded as consisting of four parts, the lacteals, the lymphatics, the conglobate glands, and the thoracic duct. Although they compose so essential a part of the animal frame, and are very generally distributed to every organ of the body, yet our acquaintance with them is comparatively of modern date. It appears indeed that some portions of them were known to Galen, but in a very imperfect manner only, while he was ignorant of their specific use, and of their destination, conceiving them to be only a branch of the sanguiferous system. Their very existence seems, after this period, to have been over-looked or forgotten, until Eustachius discovered the thoracic duct; but, although he describes its form and structure with considerable accuracy, he, like the ancients, had no conception of its specific nature, as forming a portion of a great

system of vessels, distinct from the arteries and veins; and only indirectly connected with them.

It is to Aselli that we are indebted for our acquaintance with the lacteals, as a specific and distinct system, possessing a peculiar structure, and an appropriate office; a discovery which he made in the year 1622.⁷ In the course of his dissections, he observed a series of vessels, unconnected with the arteries and veins, dispersed over the mesentery of a dog; and in consequence of the appearance of the chyle with which they were filled, he gave them the name

⁷ See his treatise, *De Lactibus*, accompanied by the singular engravings; also Sheldon, on the *Abs. Sys.* p. 20, 1. For an account of what was known respecting the absorbents, before the time of Aselli, the student may consult Bartholin, *de Lacteis Thorac.* c. 2; he remarks that Galen, Fabricius, Pao, Gassendi, and Conring saw some parts of the absorbents, but had false notions respecting them. In addition to these, we may add the names of Fallopius and Eustachius, the former of whom appears certainly to have seen some of the absorbents connected with the liver; see his treatise entitled "*Observationes de Venis*," obs. 3, op. p. 532; and the latter the thoracic duct, which he describes with considerable accuracy, as seen in a horse; *Opusc. Anat.* p. 279, O. Vesling, in *Syntagma Anat.*, describes what appears to be the lymphatics of some of the abdominal viscera; and Vanhorne, *Nov. Duct. Chyl.*, gives a plate of what is probably intended for the thoracic duct, although very incorrectly delineated. See also Haller, *El. Phys.* ii. 3. 1. and xxv. 1. 3; and Mascagni, *Prologomena*, p. 1. . 5. Bartholin, in his 4th chap. gives an account of the progressive steps of Aselli's discovery. We have a most ample and valuable catalogue of the various publications on the absorbent system, by Scæmmering, appended to his treatise, *De Morbis Vasorum Absorbentium*, occupying no less than 34 pages.

of lacteals. He appears to have formed a correct opinion, that their course is from the surface of the intestines towards the central parts of the body ; but the discovery of their termination in the thoracic duct, and of the connexion of this duct with the great venous trunks, was made by Pecquet, in the year 1651.⁸

When Aselli and Pecquet had directed the attention of anatomists to these organs, they became the subject of very general investigation, and every circumstance respecting their structure and organization was minutely examined. They are described as originating from the villi or small projections, that are attached to the inner membrane of the intestines, which from this circumstance has obtained the denomination of the villous coat. These villi are said to be composed of, or to contain, a number of extremely minute capillary tubes, which branch off or radiate from what may be regarded as the termination of the proper lacteal, a number of these tubes uniting to form the larger vessel. But although detailed descriptions, and even drawings of these parts have been made by Lieberkuhn⁹ and others, which are supposed to represent their form and structure, there seems to be still some reason to doubt of the exist-

⁸ *Experimenta Nova*, passim, cum fig. ; see also Bartholin's 5th chap. This anatomist seems to plume himself upon the circumstance of his having been the person who first saw the thoracic duct in the human subject ; but this cannot be regarded as any great advance, after it had been clearly demonstrated in the mammiferous quadrupeds ; see Pecquet, c. 6.

⁹ *Diss. de Fabrica Vill. Intest.* passim, cum Tab. 1, 2.

ence of these parts, or at least of the exact nature of their connexion with the trunks of the lacteals. The very uncertainty, however, which prevails upon the subject, is a sufficient proof of the extreme delicacy of the organs, and so far as any practical consequences may be derived from a knowledge of their anatomical structure, we may be entitled to consider them as possessed of the physical properties of capillary tubes, but connected probably with other powers, which belong to them as vital agents.¹

¹ Haller's observations on the degree of credit which we ought to attach to the descriptions that have been published of the mouths of the lacteals, as is always the case with whatever proceeds from his pen, is deserving of great attention; See Boerhaave, *Prælect.* not. 9. ad § 91. t. i. p. 181; also not. 4. ad § 103. p. 235. Since the time of Haller our knowledge of the minute anatomy of the mouths of the absorbents is no doubt increased, but still, I apprehend, that much of what is stated as actually existing, rests very much upon conjecture. We have, however, the authority of Hewson in favour of Lieberkuhn's description, although he differs from him in some minute points; *Inq.* c. 12. pt. 2. p. 171. et seq. He adduces his own experiments and preparations in support of the doctrine. Cruikshank, on the absorbents, C. 11, supports the same opinion; but he informs us, at the same time, that he has never been able to detect the orifices of the lymphatics; *ibid.* p. 60. See also Sheldon on the absorbent system, p. 32. 8; and tab. 1, 2. Beclard, *add. à Bichat*, p. 128, and Hedwig, *Disquisit. ampullac.*

On the other hand we have the drawings of Mascagni, tab. 1. fig. 1, 3. and tab. 3. fig. 1, 2, 3, 5. which do not sanction the descriptions of Lieberkuhn. It must be acknowledged that Sheldon's testimony in favour of Lieberkuhn is very direct, and perhaps ought to outweigh the negative evidence even of Mascagni. M. Magendie altogether discredits the accounts that have been published; *Journ. Physiol.* t. i. p. 3. et alibi.

The lacteals, after they have acquired a sufficient magnitude to be easily recognised by the eye, are carried along the mesentery; like the venous part of the sanguiferous system, the small branches run together to form larger branches, while these again unite, until the whole compose a few great trunks, which terminate in the lower end of the thoracic duct. The small branches frequently anastomose with each other, and in some instances, the connexions are so numerous and intricate, as to form a complete plexus. They are furnished with numerous valves, which are of a semilunar form, disposed in pairs, and with the convex side turned towards the intestines, so that, except in cases of extreme distention, they must prevent the retrograde motion of the contents of the vessels.²

Besides the peculiarity in their course, in the nature of their contents, and their numerous valves, the lacteals are farther characterized by the thinness and transparency of their coats, by which they are rendered very difficult of detection, except when they are distended with the white and opaque chyle. But notwithstanding the fineness of their texture, they would appear to possess considerable strength, so as to be capable of being distended by injections far

² The original discoverers of these vessels were aware of the existence of the valves, but they were examined with so much accuracy by Ruysch, that the merit of the discovery is not unfrequently bestowed upon him. See his *Dilucidatio Valvularum*, Op. t. i. p. 1. 13. They are very accurately described by Sheldon, on the absorbent system, p. 28.

beyond their natural dimensions, without being ruptured. When they are thus injected, the frequency of the valves occasions them to assume a jointed appearance, somewhat resembling a string of beads. They appear to be composed of two essentially distinct parts; an interior membrane, by the duplicature of which the valves are composed, and which probably constitutes a considerable part of their actual substance, and a membrane surrounding this, which may be considered as determining the bulk of the vessel, and giving its general form. To these two some anatomists have added an external peritoneal membrane, but this, strictly speaking, is no more than the general envelope which the peritoneum affords to all the abdominal viscera.³

We appear to have very unequivocal evidence of the contractile nature of the lacteals, and yet, owing, as we may presume, to the transparency of their coats, it is doubtful whether muscular fibres have ever been detected in them.⁴ Like all the vessels of

³ For a general description of the lacteals, see Haller, *El. Phys.* xxv. l. 4. § 8; Mascagni, *Vas. Lymph. Corp. Historia*, pt. 1. § 7. art. 8. p. 50, l. tab. 1. fig. 7; Sheldon, on the Absorbent System, c. 2. pl. 3, 4, 5; Santorini, *Tabulæ*, N^o. 13, fig. 3; Magendie, *Phys.* t. ii. p. 158. . 0.

⁴ Nuck has figured what he conceived to be fibres in the conglobate glands and thoracic duct; *Adenologia*; p. 35. . 8. fig. 13, 14, 19. but Mascagni supposes that what he saw referred to the adipose cells, and farther informs us that he never could detect fibres in any of the absorbents, pt. 1. sect. 4. p. 26. Cruikshank, however, has "repeatedly demonstrated fibres in the thoracic duct;" *On the Absorbing Vessels*, p. 61; and strongly advocates

any considerable magnitude, they are provided with a set of arteries, by which they are nourished and immediately connected with the vital system; no nerves have been detected as specifically belonging to the lacteals, nor have we any direct evidence of their possessing any sensitive properties.⁵

The discovery of the lymphatics was a few years posterior to that of the lacteals; for although their structure and composition are nearly similar, yet in consequence of their contents being transparent and colourless, they are less easily detected. On this account it was not until about thirty years after the discovery of Aselli that we have unequivocal evidence of their having been distinctly recognised, and it still remains somewhat doubtful to whom the honour of the discovery is to be ascribed. Perhaps the first anatomist who clearly announced them as a distinct system of vessels is Joliffe, but as he himself published no account of his own observations, his claim is not very satisfactorily substantiated.⁶ We have,

the irritability (contractility) of the absorbent system generally;
On the Absorbing Vessels, c. 12.

⁵ Cruikshank remarks that nerves are apparently distributed to the absorbent vessels, but that we cannot perceive that they are much under the influence of these nerves; p. 64.

⁶ The claim of Joliffe rests upon the testimony of Glisson, as given in his treatise on the Liver, published in 1654; Haller, *Bibl. Anat.* t. i. p. 452. He entitles one of his sections, "*De vasis aquosis, sive lymphæ ductibus ad hepar spectantibus*," and adds the following narrative; "*Incidit primum in eorum notitiam, indicio D. Jolivii, atque anno 1652, sub initium Junii, quo tempore ille doctoratus gradum adepturus, me Canta-*

however, sufficient proof that these vessels were observed by Bartholin, and by Rudbeck, nearly about the same time, but that Bartholin was the first to publish the discovery. This publication gave rise to a claim on the part of Rudbeck and his friends, which led to a controversy that was carried on for some time with considerable acrimony. It is scarcely in our power, after this lapse of time, to form a decisive judgment on this question, but from the documents which we possess, I think we may conclude, that the lymphatics were first clearly discovered by Rudbeck, and that Bartholin had some intimation of the discovery; this he appears, rather disingenuously, to have concealed; yet we may allow that he had considerable merit in profiting by the hint, and in pursuing the investigation, with the skill and address which he displayed on all points connected with

brigiæ in eum finem convenerat. Asseruit nempe, dari vasorum quartum genus, à venis, arteriis, et nervis planè diversum; idemque ad omnes ut plurimas saltem corporis partes distribui, humorem aquosum in se complecti. Addebat porro, se in compluribus animalibus eorundem ductum investigasse, in artubus, scil. testiculis, utero, aliisque etiam partibus, certo sibi constare, liquorem in iis versum mesenterium tendere, et particulatim ad initium sive radicationem ejus." c. 31. p. 319. Glisson, notwithstanding the designation which he gives to these vessels, afterwards expressly states; "quod ad hepatis negotium nihil spectare viderentur; neque enim dictus Jolivius, quenquam horum ductuum inde proficisci etiamnum monuerat." Haller does not appear to have thought Joliffe's claim to discovery to have possessed much weight; he remarks concerning it; "*Vasa lymphatica hic (Glisson) ut alii Angli, Jolivio tribuit inventa, ignoto inter incisores nomini.*" *Bibl. Anat. ut supra.*

anatomy.⁷ The peculiar nature of these vessels, and their supposed importance in the operations of the animal œconomy, soon attracted the attention of all the anatomists of the age, and from that period until our own time they were successively detected in the different parts of the body, and in the different classes of animals. The labours of Wm. Hunter and of Monro Sec. were particularly directed to the examination of the absorbent system, and some of the

7 Those who are desirous of examining into the respective merits of Bartholin and Rudbeck may consult Haller, not. 4. ad Boer. Præl. § 121. t. i. p. 277. 9. or Bibl. Anat. t. i. § 378. p. 400. et seq. and sect. 415. p. 447. et seq. where he will find a list of the various publications to which the controversy gave rise; and still more, El. Phys. ii. 3. 1. where the history of the discovery is detailed with the author's accustomed accuracy, and with that correct distribution of justice to the respective claimants, for which he is so highly and justly celebrated. The result of the inquiry has, I acknowledge, produced upon my mind the impression which is stated in the text. Bartholin was certainly a skilful and active anatomist, to whom the science lies under many obligations, but I think that his works betray the ambition of being regarded as a great discoverer, a spirit, which, when it once takes possession of the mind, is too apt to blunt the finer feelings of honour and integrity. Bartholin's own statement is briefly made in his Anat. Reform. p. 621, 2. There is reason to suppose that Bogdan's angry treatise was written under the immediate inspection of Bartholin. Haller obviously inclines to the part of Rudbeck; he says, "videtur ex his ipsis datis verus novorum vasorum inventor fuisse;" see also the remainder of the paragraph in Bibl. Anat. t. i. p. 447, 8; see also El. Phys. ii. 3. 1. p. 161, 2. Boerhaave, in his work "Methodus Studii Medici," gives a history of the successive discoveries that were made respecting the lymphatics; C. 2. de vasis lymphaticis, t. i. p. 443. et seq.

anatomists of the present day are still engaged in discussing the nature of their action, and the relation which they bear to the other parts of the system.

The lymphatics appear to be very similar to the lacteals in their structure, and in the nature of their constituent parts, being composed, like them, of a fine and transparent, but firm and elastic substance, provided with numerous valves, and forming frequent anastomoses. They seem likewise to possess a similar degree of contractility, although from the nature of their contents, it is not so easy to demonstrate it by actual experiment, and they are also analogous to them in their principal function, and in their ultimate destination. But they differ from the lacteals in their situation, and in their contents, for whereas the lacteals are confined to the mesentery, and serve only to convey the chyle, the lymphatics are found in almost every part of the body, and are filled with a transparent and colourless fluid, which, as its name imports, was supposed to consist principally of water. The origin of the lymphatics appears to be from the various surfaces of the body, external as well as internal,⁸ and partly from the degree in which we are actually able to trace them by anatomical injections, and partly from observing changes to be pro-

⁸ Magendie, *Physiol.* t. ii. p. 175, remarks, "On ignore la disposition que les lymphatiques ont à leur origine; on a fait à ce sujet beaucoup de conjectures, également dénuées de fondement." Injections demonstrate that they arise from minute branches which can be traced into the neighbourhood of the various surfaces, but beyond this we have no certain information.

duced in various organs, which we can only explain by the power of absorption, we are induced to suppose that they exist in every part of the body.¹

Their great trunks are arranged into two principal series or systems, one near the surface, and the other more deeply seated, and we find that, for the most part, they follow the course of the great veins:

¹ Haller entitles one of his sections, *El. Phys.* ii. 3, 4. "Ubi nondum visa sunt;" it would seem that, at the period when he wrote, there were very few parts of the human body in which they had not been detected; and since his time this number is still farther diminished. It appears, however, that there are some organs, more particularly the brain, the spinal cord, and the organs of sense, which are at least much less plentifully supplied with absorbents than the other soft parts; indeed it may be doubted whether we have any unexceptionable evidence of their having been seen in these organs. M. Magendie, writing in 1817, says, "C'est en vain qu'on a cherché jusqu'ici ces vaisseaux dans le cerveau, la moelle épineuse, leurs envelop, l'œil, l'oreille interne," &c.; *Physiol.* t. ii. p. 174; see also *Journ. de Physiol.* t. i. p. 3. 1821. Mascagni gives us a view of a few small lymphatics which he had discovered in the brain; tab. 27. fig. 1, 2, 3. Are we to consider the above fact as a proof, that the brain and nerves are less disposed to undergo that gradual exchange of particles which has been so frequently referred to? For descriptions and views of the lymphatics, see Haller, *El. Phys.* ii. 3. 2. et seq.; Hewson, *Inq.* c. 3. and pl. 3. 6; Mascagni, *Vas. Lymph. Hist.* pt. 1. sect. 7. p. 37. et seq.; and tab. 4. et seq.; Cruikshank, on the Absorbents, p. 148. et seq.; Sæmmering, *Corp. Hum. Fab.* t. v. p. 388. et seq. The origin of the lymphatics has generally been supposed to be still more obscure than that of the lacteals; we have, however, an account by Watson, which bears the marks of fidelity, of his being easily able to detect their open mouths on the surface of the bladder; *Phil. Trans.* for 1769; pl. 16.

Whether this depends upon any necessary connexion which takes place between these sets of vessels, during their course from the superficial to the central parts of the system, or whether they are lodged near each other for the mere purpose of mechanical accommodation, we are, perhaps, not able positively to determine; but the latter seems the more probable supposition. The main branches of the lymphatics are finally reduced to three or four great trunks, which, like the lacteals, terminate in the thoracic duct.

This duct is the ultimate destination of all the lacteals and lymphatics; it is a vessel of considerable size, which lies in the neighbourhood of the spine, running in a somewhat tortuous course, from the third or fourth dorsal vertebra, to about half an inch above the trunk of the left subclavian vein. It is then bent down into the form of an irregular arch, and opens into this vessel, nearly at its union with the jugular of the same side. There is considerable irregularity in the form of the thoracic duct; in a majority of cases, it is composed of a single trunk; occasionally there are two trunks, which are not very dissimilar from each other in their dimensions, while not unfrequently we have one or more small trunks which pass in the same direction with the main duct, which are generally united to it in some part of its course, or, in some cases, are separately transmitted into the subclavian vein. Although this formation of the thoracic duct and its supplementary appendages, does not affect its physiological functions, and is no more than a mere anatomical variation, yet it is

of importance to be aware of it in our experiments on the absorbent system; for it appears that the earlier anatomists were not unfrequently induced to form false conclusions on this subject, by supposing that they had intercepted the transmission of the chyle from the absorbent to the sanguiferous system, when they had merely prevented it from passing along the main trunk of the thoracic duct.² Excepting in its greater size, it is not essentially different from the other absorbent vessels; its coats are thin and transparent, yet possessed of considerable strength and elasticity; it is furnished with numerous valves, and appears to possess a remarkable degree of contractility.³

The conglobate or lymphatic glands compose a conspicuous portion of the absorbent system. They are met with in different parts of the body, always connected with the lacteals or the lymphatics. They

² See the observations and experiments of Sir A. Cooper in *Medical Records and Researches*, p. 86. et seq. where he shows that when the duct is obstructed either by mal-conformation, or by a ligature, the chyle still finds its way into the veins. See also the paper of M. Magendie, in his *Journ. t. i. p. 21.*

³ For the description and views of the thoracic duct, see Haller, *Prim. Lin. ch. 25. § 565*; *Op. Min. t. i. p. 586. et seq. tab. 11, 12*; and *El. Phys. xxv. 1. 10. .3*; Albinus, *Tab. Vas. Chylif*; Cheselden, *Anat. pl. 26*; Portal, in *Mem. Acad. pour 1770*, and Sabatier, *pour 1786*; Haase, *de Vas. Cutis et Intest. Abs. tab. 2. and tab. 3. fig. 1.*; there are two valuable treatises on this organ in Haller, *Disp. Anat. t. i. by Bolius and by Saltzmann*; Mascagni, *pt. 1. sect. 7. art. 8. p. 52. and tab. 13, 15, 19*; Sheldon, *pl. 5*; Cruikshank, *p. 166. .176*; Magendie, *Physiol. t. ii. p. 160.*

are of various sizes, sometimes simple, sometimes in groups or clusters, and although their use is not understood, we may presume that they serve some important purpose, from the circumstance of every absorbent vessel, during its course, passing through one or more of these glands.⁴ They are very numerous in the mesentery as connected with the lacteals, and as attached to the lymphatics; there are large clusters of them in the groin, the neck, and the axilla, as well as in the course of the greater lymphatic trunks, not far from their termination in the thoracic duct.

It is, however, only in the mammalia, or the animals which most nearly resemble them in their structure and functions, that these glands are found so abundantly; even in birds they are rare, and still more so in fishes.⁵ Although we are not acquainted

⁴ It has been questioned how far this remark is literally true; Hewson affirms that he has injected lymphatics, which have been unconnected with any glands; Inquiry, pt. 2. p. 44, 5; and the same statement has been made by others. But Mascagni, in his numerous injections, has never met with this circumstance, and expresses himself as if he doubted the correctness of the observation; Vas. Lymph. Hist. pt. 1. sect. 4. p. 25; see also Gordon's Anat. p. 74.

⁵ The researches of the modern anatomists have proved that the absorbent vessels exist in the great classes of the mammalia, birds, amphibia, fishes, and insects. Blumenbach observes, that the heart and the circulation of the blood are always co-existent with the absorbent system, and that although animals which are without red blood appear to absorb fluids, yet that it is not done by the same kind of vessels as in animals that possess red blood; Comp. Anat. by Lawrence, p. 253. I apprehend that the

with the nature of the function which is exercised by these glands, we may fairly presume that they serve, in some way or other, to the completion or the perfection of the absorbent system, as they are found principally in the higher orders of animals. In those of an inferior description, we have the vessels without the glands, while in those of a still lower order, neither the vessels nor the glands can be detected, so that the process of absorption must be carried on by some more simple apparatus. There has been the same kind of controversy respecting the structure of the conglobate, as of the conglomerate glands, whether they contain cells, or whether they consist of a mere congeries of vessels. Nuck⁶ and the earlier anatomists generally maintained the former opinion, while

first part of this remark cannot be considered as perfectly correct, since they have been detected in the silk-worm, Sheldon on the Absorbent System, pt. 1. p. 28; and the Echinus Marinus, Monro on Fishes, p. 125. . 8. tab. 44. We may conceive that the whole process of growth and nutrition, in all its parts, depends upon a species of absorption even in the lowest orders of animals, although there are many considerations which would lead us to suppose, that it consists in little else than mere mechanical imbibition, quite distinct from proper vascular action. Dr. Fleming's account of the comparative anatomy of the absorbents may be perused with advantage, although it must be regarded rather as a popular, than as a technically correct view of the subject; Zoology, t. i. p. 338. Mascagni, speaking of the classes of animals that possess a lymphatic system, says, "*hoc forsan donantur alia animalia corde et vasis sanguineis destituta*," p. 2.

⁶ Nuck gives us the results of his own injections in a simple and candid manner, accompanied with rough engravings; C. 2. p. 30. et seq. fig. 9. . 12.

the more recent authors, on the contrary, for the most part, incline to the latter.⁷ Hewson informs us that "each gland is a congeries of tubes consisting of arteries, veins, lymphatic vessels, and nerves, connected by the cellular substance."⁸ Mascagni gives an account of his own observations on the glands, when they are examined, after having been injected with wax or glue; "*apparebit lymphatica ... dividi, invicem coire, flecti, extenuari, dilatari, cellas efformare, rursus constringi, mutuo demum commixtione surculorum, præsertim vero ramis in cellas immissis, indeque inductis, amplo commercio donari.*"⁹

⁷ Cruikshank, however, argues in favour of the cellular texture, c. 14. and Mr. Abernethy appears to have clearly proved that this is the case in the whale; *Phil. Trans.* for 1796, p. 27. et seq.

⁸ *Inquiry*, v. iii. c. 2. pl. 2.; see also Beclard, *add. à Bichat*, p. 231; *Monro Tert. Elem.* v. i. p. 558; *Werner and Feller, Vas. Lact. and Lymph. Descrip.* tab. 2; they delineate the gland as distinctly consisting of a network of vessels, but I cannot but suspect that the drawing is somewhat exaggerated; *Haller, El. Phys.* ii. 3. 16.. 27. gives a very full account of the structure, situation, and supposed uses of these glands; see also *Boyer, Anat. t. iii. p. 243.. 257.* I have already had occasion to remark p. 323. that Sylvius was the first who distinguished these glands from those that are more immediately concerned in secretion, and appropriated to them the name of conglobate, while he styled the latter conglomerate; these names have been generally employed, although not very correct or appropriate. For views of these glands, see *Mascagni*, tab. 1. fig. 8.. 12, tab. 2. fig. 4.. 8. tab. 4. fig. 2. tab. 8, 16, 26; *Cruikshank*, pl. 3; *Sheldon*, tab. 3, 5; *Parr's Dict. Absorbents* 1, 2, 3.

⁹ *Vas. Lymph. Hist.* pt. 1. sect. 5. p. 31. There has been considerable difference of opinion respecting the anatomical relation between the conglobate glands and the nerves; *Malpighi* and *Nuck* thought that these glands were plentifully supplied with

§ 2. *Office of the Absorbent System.*

The office of the absorbents is literally expressed by their name ; it consists in receiving or taking up certain substances, and in transporting them from one part of the body to another. The substances which are thus taken up, may be referred to two only, the chyle and the lymph, the former being received by the lacteals, and the latter by the lymphatics. The immediate object of the action of the two sets of vessels is also essentially different, that of the first being to convey a fluid from the part where it is formed into the blood, in order that it may directly serve for the nutrition of the body, the latter serving, in the first instance, to remove what is useless or noxious, and to dispose of it in such a manner, that it may either be applied to some secondary purpose of utility, or be finally discharged from the system.

Although there is some uncertainty respecting the anatomical structure of the mouths of the lacteals, and there is considerable difficulty in explaining the mode in which the chyle enters them, we can have no doubt that they are so dispersed over the surface of the intestines, as to be able to receive the chyle when it is presented to them. By their contractile power, assisted by the mechanical action of the valves, and probably by other causes, which will be considered

nerves ; Hewson that they have few nerves, and that they only become sensitive when affected by acute inflammation ; Inquiry, pt. 3. p. 52 ; Mascagni informs us that he never detected nerves distributed to these glands, p. 30 ; he further states this to have been the case with Walter. Gordon remarks that no nerves have been discovered accompanying these vessels ; Anat. p. 77.

presently, the fluid, when it has once entered the vessels, is necessarily propelled from their extremities towards their trunks, until at length it arrives at the thoracic duct. The action and functions of the lymphatics do not appear to be essentially different from those of the lacteals; we have, however, a still less distinct conception of their extremities and of the mode in which they receive their contents; when the lymph has once entered them, we may presume that it is propelled forwards precisely in the same manner with the chyle. There is, however, one circumstance in which these two sets of vessels would appear to differ from each other, at least in degree; that whereas the lacteals seem to be capable of receiving nothing except chyle, which they, in some way or other, possess the power of selecting from the heterogeneous mass of matter through which it is diffused, and with very few exceptions, reject every thing else that is presented to them; the lymphatics, on the contrary, possess the distinguishing property of taking up, as occasion may require, every substance that enters into the composition of the body, as well as extraneous and heterogeneous matters of various kinds, that are accidentally, or intentionally placed in contact with their mouths. This is not only the case with our various fluids and solids, which are composed of similar elements, and might therefore be conceived to be readily convertible into each other, but they have the power of absorbing the earth of bones, and even of taking up various medicinal agents, and carrying them into the system, so as to enable

them to produce the same effect upon the functions, as if they had been received into the stomach.

With respect to the thoracic duct we have no reason to suppose that there is any thing specific in its action, or that, except in its size, it differs from the other absorbent vessels. Its particular office appears to be that of serving as a reservoir in which the chyle and lymph may be deposited, for the purpose of being gradually transmitted into the sanguiferous system, as there is some reason to suspect, that injury would ensue if too large a quantity of this fluid were poured into the veins at any one time. It is not improbable that a certain degree of retardation is necessary, in order that the contents of the absorbent system may be more completely assimilated, before they are mixed with the blood, which could not have been so conveniently effected, without the intervention of a receptacle similar to the thoracic duct.

From the above remarks it appears that we can have little doubt respecting the use of the vascular part of the absorbent system, but this is not the case with its glandular appendages. It can scarcely indeed appear surprising that we are unable to explain their use, while their structure is still involved in so much obscurity, and yet, on the other hand, it may be said that we know so little of glandular action, or of the change which it produces upon the fluids that are subjected to it, that we should rather attempt to elucidate the subject by physiological, than by anatomical investigations. The most probable opinions that have been entertained upon the point are, either

that these glands are proper secreting organs, and are intended to prepare a peculiar substance, which is mixed with the chyle and lymph, or that they offer a mechanical obstruction to the progress of these bodies, by which means their elements are allowed to act upon each other, and thus to produce some necessary change in the nature of the fluids which pass through them.¹ The examination of the contents of these vessels does not enable us to decide this question, nor am I acquainted with any considerations, anatomical or physiological, which appear to have much weight in directing our determination.

We must, however, suppose that some important

¹ Richerand supposes that the glands tend to assimilate and animalize the chyle, and to separate the heterogeneous matters from it, but this opinion is entirely conjectural, *Elem.* p. 153; this is nearly the opinion of Blumenbach, *Inst. Phys.* § 425, 442. Mascagni supposes that they serve to detain the fluid and to mix its parts together; this is proved by the difference in the nature of the fluid before and after it passes through the gland; *Pt. I. sect. 5. p. 33.* I do not, however, perceive that it is stated in what this difference consists. M. Magendie very candidly confesses his ignorance upon the subject; *Phys. t. ii. p. 166, 201.* Haller supposes that the functions of these glands are more important in the young than in the adult animal; principally, as it would appear, resting his opinion upon their greater size, and upon their containing a greater proportion of fluid in the former case, *El. Phys. ii. 3. 25.* It is natural to suppose that during the growth of the body, a greater quantity of nutritive matter will be conveyed to the blood, which must necessarily pass through these glands, whatever use we may ascribe to them. Mascagni agrees with respect to the fact of their being larger and more turgid in youth; *Vas. Lymph. Hist. pt. 1, sect. 5. p. 33.*

change is effected by their means, from the fact mentioned above, that every absorbent, during some part of its course to the thoracic duct, passes through one or more of these glands. But the same mode of reasoning might lead us to conclude, that although the absorbent glands are necessary to the existence of the higher orders of animals, they are not so for the purposes of nutrition and growth generally, as it appears that there are large classes of animals, which resemble the mammalia in many of their nutritive functions, and in the vascular part of the absorbents, which are without any lymphatic glands, or are very sparingly furnished with them.² It is not easy to point out any circumstances that belong exclusively to the mammalia, which can assist us in explaining the necessity for these appendages to their lymphatic system.

Ever since the complete discovery of the lacteals and the lymphatics, it has been a general opinion, both with anatomists and physiologists, that their appropriate office was absorption; but it has been a very warmly contested point, whether this operation was exclusively performed by these vessels. The ancients, who were ignorant of their existence, supposed that the process of absorption and transmission, so far at least as they had any definite ideas upon the subject, was performed by the veins; and in modern times, after the full discovery of the extent and properties of the lymphatic system, it was still sup-

² Blumenbach's *Comp. Anat.* by Lawrence, ch. 13. p. 256.

posed, that the veins assisted in the process, and even, in some cases, were the principal agents. This was almost the universal opinion until the middle of the last century,³ and is the doctrine which was strenuously maintained both by Boerhaave⁴ and by Haller.⁵

The arguments which were employed by these distinguished physiologists, as well as by the other ana-

³ It would appear that Malpighi conjectured that the lymphatics originated from the glands; *De Struct. Gland.* p. 3. Nuck, in consequence of the results of some of his injections, was led to think that they were immediately connected with the blood-vessels, *Adenographia*, ch. 4, and this opinion was after that time very generally embraced. *Monro Sec.* in his treatise, *De Venis Lymph. Valv.* p. 14.. 21, gives a most ample list of references to the authors who adopted this opinion, including, indeed, almost all the anatomists of eminence previous to the period when he wrote.

⁴ *Prælect.* § 103. t. i. p. 234; also § 247. et seq. t. ii. p. 303.

⁵ In note 1 to § 106 of *Boer. Prælect.* t. i. p. 241, he gives a statement of the question, and a list of the authors who have defended the doctrine of venous absorption; see also note 1 to § 245. t. ii. p. 197, and *El. Phys.* ii. l. 28. *M. Magendie* enumerates *Ruysch*, *Boerhaave*, *Meckel*, *Swammerdam*, and *Haller*, as the most powerful supporters of this doctrine; *Physiol.* t. ii. p. 238. *Hoffmann* appears to have been one of the earliest writers who decidedly maintains the opinion that absorption is exclusively carried on by the lymphatics; *Med. Rat. Lib.* 1. sect. 2. ch. 3. Some of the most direct experiments in favour of venous absorption are those of *Kaaw Boerhaave*, who informs us that fluids injected into the intestines, under certain circumstances, were afterwards detected in the mesenteric veins, *De Perspir.* § 469. p. 202, 3; but the experiments are related very briefly, and in so general a way, as not to admit of our placing much confidence in them.

tomists who had preceded them, may be all reduced to two classes: the first, and indeed those on which they were disposed to place most confidence, were the results of experiments, which seemed to demonstrate, that injections of various kinds were capable of passing from one set of vessels to the other, thus indicating that there existed a natural and direct communication between them. The most skilful anatomists of that period generally admitted this to be the case; and I do not find that the correctness, either of the experiments or of the deduction from them, was ever called in question. The other series of arguments, which, although indirect, were supposed to be of great force, was derived from the anatomical fact, that in many parts of the human body, where the effects of absorption are sufficiently obvious, no lymphatics had been detected, and still farther, that there are large classes of animals, and those possessed of an organization in many respects similar to the mammalia, where the absorbent system appears to be wanting. In these cases, therefore, it seemed that we were reduced to the necessity of supposing that absorption was effected by the veins, and if it were so in one case, there was no difficulty in extending it to the rest, especially so far as related to other parts of the same animal.

The doctrine of venous absorption was first formally attacked by Wm. Hunter and Monro Sec., who seem, nearly about the same period, to have entered upon the regular investigation of the subject.⁶ With

⁶ It is a painful, yet necessary task, which is imposed upon

respect to the mode of reasoning that had been employed by their predecessors, they endeavoured, by

the historian of science, to notice those personal controversies which occasionally take place, and which frequently originate in the right to certain discoveries. Few have been more acrimonious than the one alluded to in the text; the respective claims of the two parties may be found in Wm. Hunter's *Med. Com.* and in *Monro's Observations Anat. and Phys.*, and the sixth chapter of his *Treatise on the brain*. I believe I may assert, that the sentiments of the great majority of the men of science were in favour of the former. *Monro*, in his *Inaugural dissertation*, published at *Edinburgh* in 1755, p. 25, and still more explicitly in his *Treatise on the lymphatics*, published at *Berlin* in 1757, c. 12. p. 556, clearly states his doctrine respecting the non-absorption of the veins; but there appears ample testimony to prove, that *Wm. Hunter* had, for some years previously, publicly taught the same doctrine in his lectures, in the most decisive and unequivocal manner. It was a very natural, and even a very laudable feeling in the present *Professor Monro* to decline entering into the merits of a discussion in which his father's character was involved, but certainly, as he thought proper to reprint *Black's* letter to his father, *Elem. v. ii. p. 459*, justice demanded that he should likewise have inserted the two which the same eminent philosopher subsequently wrote to *Wm. Hunter*; *Med. Com. p. 22. . 5*. Although the connexion of *Dr. Baillie* with the *Hunters* might render him a suspicious evidence, yet, on the other hand, his thorough knowledge of the point in discussion, as well as his well-known candour and impartiality, must render his testimony of considerable value. After speaking of *Wm. Hunter's* investigations, *Dr. Baillie* remarks; "This discovery has been claimed by a celebrated professor of anatomy in *Edinburgh*; but I shall avoid entering into the dispute. It is enough to say, that *Dr. Hunter* taught this doctrine in the year 1747, six years before the professor declares himself to have made the discovery. *Dr. Hunter* has, therefore, an undoubted claim to priority, whatever praise may belong to

carefully repeating the experiments, and by observing attentively every circumstance connected with them, to show that where injections had passed between the veins and the absorbents, some rupture or extravasation had taken place, and that when the process was performed with proper care, and the necessary allowance made for unavoidable accidents, the connection between the sanguiferous and absorbent systems could not be substantiated. They afterwards examined, with much assiduity, the various parts of the body, where absorbents had not been previously detected, and they were successful in discovering them in so many new situations, that it appeared to be a fair inference, that every part is provided with a proper absorbing apparatus, although the peculiar texture and appearance of the vessels rendered them difficult to be demonstrated.⁷ They were ably seconded in their labours by various anatomists, both in this country and on the Continent, who extended their observations to other classes of animals, and discovered an absorbent system in many of them, where it had not been previously suspected. Among those who were the most successful in this department we may class Hewson, who shared with Monro the merit of having first observed the ab-

any other person for having made the same discovery without assistance."

⁷ See Cruikshank on the absorbent system, *Introd.* for an account of Wm. Hunter's researches on this subject; Monro states the result of his investigation in his treatise, *De Ven. Lymph. Valv.* p. 103.

sorbents in fishes, and Mascagni, Sheldon, and Cruikshank.⁸

In addition to these investigations, which must be considered as principally intended to counteract the arguments employed by preceding anatomists, the main scope of which was to show, that the structure of the body did not render it necessary for us to have recourse to venous absorption, experiments were performed for the direct purpose of proving that the veins are incapable of performing this function. Some of the first and most decisive of these were executed by J. Hunter. They consisted in filling portions of the small intestines with milk, or some similar kind of fluid, and retaining it there, so as to produce a degree of distention of the part, and afterwards examining whether any of the fluid that was employed had entered the veins of the intestines. This was said in no instance to have occurred, and

⁸ The controversy which took place between Monro Sec. and Hewson, respecting the priority of discovery on this point, was no less acrimonious than the one mentioned above between Monro and Hunter; see Hewson's Inquiry, App. to the 1st. vol. We may presume that the general sentiment in this case was in favour of Hewson, as the Royal Society presented him with the Copley medal in 1769, for his experiments on this subject, which they would scarcely have done, had they not supposed them to be original. Hewson's communications to the Royal Society are in their volumes for 1768, p. 217. et seq.; a paper on the lymphatics of birds, in which he mentions that he had also discovered these vessels in a turtle and in fishes; and for 1769, in which we have two papers, the first on the lymphatics in a turtle, p. 198. et seq. and the second in fishes, p. 204. et seq.

hence it was argued, that as venous absorption did not take place, under circumstances which seemed favourable to its action, the veins were not possessed of this power.⁹

In consequence of the number of observations and experiments of this description, which were, from time to time, laid before the public, and from various physiological and pathological considerations, all of which appeared to concur in confirming the doctrine of absorption being exclusively performed by the lacteals and lymphatics, the old opinion was daily losing ground, until at length the modern doctrine seemed to be fully established, in so much that there was perhaps no hypothesis in the whole range of physiological science, which appeared to rest on a firmer foundation, than that of the non-absorption of the veins.¹

It affords us, however, a striking illustration of the uncertainty of all human knowledge, and the mutability of all opinions, even those that seem to be founded upon the most direct and unequivocal evidence, that shortly after this unity of sentiment

⁹ The experiments are related in *Med. Comment.* c. 5. p. 42.. 8. Cruikshank gives an abstract of them in c. 5. p. 21. et seq.; he remarks, "these experiments appear to me perfectly conclusive."

¹ We have a very judicious summary of the opinions that had been successively adopted on the subject of venous absorption, given us by Mascagni, in Part 1. Sect. 2 and 3, of his great work. He considered the doctrine of absorption being exclusively performed by the lacteals and lymphatics as firmly established.

had taken place among physiologists, and when all controversy had ceased, or when the only subject of discussion was to ascertain in what degree the different anatomists had contributed to the establishment of the doctrine, it was again called in question by one of the first authorities of the age; direct experiments were adduced, that bore the marks of great ingenuity in their contrivance, and accuracy in their execution, the results of which were perhaps at least as decisive in favour of venous absorption, as the former had been in support of the opposite doctrine. The labours of M. Magendie on this subject, to which this observation refers, come to us in such a form, as to entitle them to the highest attention, and although on all topics connected with the animal œconomy, where we are principally to depend upon experiments performed on living animals, it is necessary to be extremely cautious in forming our judgment, yet it is in a great measure, upon such facts, when fully established, and clearly developed, that our ultimate conclusions must be founded.

The arguments by which the Hunters endeavoured to establish their position, that the process of absorption was exclusively carried on by the lymphatic vessels, were derived partly from general considerations, connected with the analogies of the other parts of the animal œconomy, and partly from experiments performed for the express purpose of proving their hypothesis. Of these latter an account has been given above, and I shall only further refer to them for the purpose of remarking, that they were con-

ceived to be so convincing and so perfectly satisfactory, that they were generally acquiesced in by anatomists and physiologists, almost without a single exception.

The considerations of a more general nature, which were adduced to prove the exclusive function of the lymphatics, were principally two; in the first place, the analogy which they bore to the lacteals, in their physical properties, their anatomical structure, and their destination; and as it was admitted that absorption was the appropriate office of the lacteals, so it was concluded that the same office must be performed by the lymphatics. In the second place, a variety of facts were adduced, in order to show that when the system became affected by the introduction of any noxious substance into the circulation, a morbid state of the lymphatics might be traced from the part where the injury was inflicted, along their trunks, towards the thoracic duct; thus proving both that some injurious substance had been received, and that it had been conveyed by these vessels. It was then argued, that if these vessels possess the power of absorption in certain cases, and if we know of no other function which they perform, it may be inferred that the whole business of absorption is carried on by them.

The force of the analogical argument appears to be still admitted; it is therefore upon the experiments that have been lately performed, that the opposite opinion is principally founded; it will consequently be necessary for us to examine how far they are so direct, as decisively to prove the point in discussion;

or whether they are more to be regarded, as tending to weaken the force of the experiments, that were formerly adduced in support of the contrary doctrine. He shall find upon inquiry that they bear upon both these positions; and among those which must be classed under the first denomination, there is one which is detailed by M. Magendie, as performed by himself, in conjunction with M. Delille, which seems to be so well contrived, and the results of which, as related by the author, are so unequivocal, that it would almost appear sufficient singly to substantiate the hypothesis. The experiment consisted in dividing all the parts of one of the posterior extremities of a dog, except the artery and the vein, the former being left entire for the purpose of preserving the life of the limb. A quantity of a poisonous substance, the *Upas tiuté*, was then applied to the foot, when, in the short space of four minutes, its effects were rendered visible upon the functions of the animal, and in ten minutes it proved fatal. In this case it was supposed that there could be no conceivable communication, by which the substance could be conveyed from the extremity to the central parts of the system, except the vein; and hence the conclusion seemed to follow irresistibly, that the vein was, in this case, the absorbing vessel. In order to render the result still more unexceptionable, a second experiment was tried, in which small leaden tubes were introduced into the artery, and the vein; and, after being secured in their places by ligatures, the vessels themselves were completely divided, so that the two streams of the arterial and venous

blood, respectively, were now the only channel of communication between the extremity of the limb, and the body of the animal; yet, under these circumstances, the poison produced the same effect as in the former case. It will be unnecessary to adduce any more experiments of this description; for the object was so clearly defined, and the result so unequivocal, that it has been generally supposed that there could be no room for objection, except such as might depend upon the want of skill or accuracy in the operator, or upon certain causes which always interfere with experiments upon the living body, but to which the one in question does not appear to be particularly obnoxious.²

And with respect to the other description of experiments, those which were performed for the purpose of contradicting, or invalidating, the statements which had been brought forwards by J. Hunter, we have equally direct evidence in favour of the modern doctrine. We are told that experiments, similar to those of Hunter, have been made by M. Flandrin, but with

² There is, indeed, a circumstance connected with the experiment, which seems to require further explanation, or in which the expression employed is somewhat ambiguous. In speaking of the mode in which the poison was applied, M. Magendie says, that it was "enfoncé dans la patte" of the dogs; now if this implies that a wound was made in the part, into which the substance was inserted, it may be conceived that a portion of it would be, in the first instance, introduced into the veins, and carried directly to the heart. This, however, it is evident, would not be a case of absorption, but simply of the power which certain bodies possess of uniting with the blood, and still retaining their specific properties.

contrary results ; and that M. Flandrin's experiments were repeated by M. Magendie, and found to be correct.³ We have here, therefore, the opposing testimony of men, both of them eminent for their general science, and especially for their address in experimental researches. If they both stood upon equal ground, we might fairly estimate the authority of Hunter as equal to that of his opponent ; but when we reflect upon the advantage which, from various causes, accrues to every succeeding experimentalist, over those who have gone before him, the balance of opinion must necessarily incline to the latter. In this case moreover, we are informed that the experiments of M. Magendie, and his friends, were considerably more numerous than those of Hunter ; and I may farther observe, that receiving each of them as they are given to us by their respective authors, and supposing that we may repose with equal confidence upon their correctness, the latter would appear to have the advantage, not merely in their greater number, but likewise in the mode in which they were performed. I do not, however, feel disposed to assent to the modern doctrine of absorption, merely upon the faith

³ *Physiol.* t.ii. p.181, et seq. In taking a view of the controversy, respecting the absorbing power of the veins, I have endeavoured to regard it, as little as possible, as a mere question of authority. I would not, therefore, assert that I conceive the experiments of Magendie to be, in themselves, superior to those of Hunter ; but as they appear to have been much more numerous, and are of later date, we may suppose them to be entitled to more credence.

of experiments, until they have been further repeated, and diversified; but, in the mean time, we may go so far as to assert, that venous absorption is neither impossible, nor, perhaps, antecedently improbable; and that with the evidence which we now have in its favour, we can admit of no physiological hypothesis, or train of reasoning, which necessarily involves its non-existence⁴.

⁴ A summary of the experiments and arguments of M. Magendie, in favour of the doctrine of venous absorption, are contained in his Memoir "*Sur les Organes de l'Absorption*;" *Journ. Physiol.* t. i. p. 18, et seq., an abstract of which is given in his *Elem. Phys.* t. ii. p. 238., 243. He concludes his observations with the three following positions: "1. It is certain that the chyloferous vessels (lacteals) absorb chyle. 2. It is doubtful whether they absorb any thing else. 3. It is not proved that the lymphatic vessels possess the power of absorption, and it is proved that the veins have this power." The same conclusion, so far as respects the mesenteric absorbents, is the direct inference from the late experiments of Tiedemann and Gmelin, of which an account will be given below. Bichat states the arguments that have been urged against venous absorption, and admits their force; yet he is not disposed to decide against the doctrine; *Anat. Gen.* "*Syst. Absorb.*" t. ii. p. 104, 5. His commentator, Beclard, embraces the opinion more decidedly, p. 130. One of the most intelligent defenders of the doctrine, among the contemporaries of Hunter, was Meckel; see his treatise, *De Fin. Ven. ac Vas. Lymph.*, written in 1772. We have a fair view of the state of the question, as it stood between 30 and 40 years ago, in the *Edinburgh System of Anatomy*, v. iii. pt. 6. sect. 8. § 2. p. 236., 245. See also some sensible observations in Hewson's *Inq.* v. 1. c. 9. It appears, both from his work and from Monro's, that the result of injections was the principal argument employed at that time to prove absorption by the veins, an argument which

But, whatever conclusion we may be induced to form respecting the office of the veins, or the share

would be most satisfactory, could we prove that no rupture or extravasation had taken place; but in consequence of the perpetual liability to such accidents, it must be regarded as of a very equivocal nature. The analogy of the lymphatics with the lacteals, and the effect of the absorption of deleterious substances, are the proofs on which Hewson principally rests his opinion. This circumstance is also insisted upon by Cruikshank, who remarks, that in the absorption of poisons, it is the lymphatics, and not the veins, that are inflamed; On the Absorbent System, p. 28. A remark of the same kind is made by Mr. Bell; *Anat. v. ir.* p. 303, he says, indeed, that the veins do occasionally become inflamed, but that they are much less liable to inflammation than the lymphatics. It has been urged, as a proof of absorption being carried on by the lymphatics, that this process continues for a considerable time after the circulation has ceased. Bichat limits this period to two hours; *ubi supra*, t. ii. p. 118; but it is supposed by many anatomists to remain for a considerably greater length of time. It is necessary to observe, that although M. Magendie conceives that the lacteals have the power of absorbing chyle, and probably are the principal agents in this operation, yet he performed a series of experiments, in conjunction with M. Delille, the results of which convinced him that the mesenteric veins also possess this power; see *Journ. Physiol.* t. i. p. 23. et seq; also, *Elem. Physiol.* t. ii. p. 183..5. The experiment consisted in detaching a portion of the small intestines from the remaining part of the canal, in dividing all its lacteals and its blood-vessels, except one artery and one vein; a deleterious fluid was then injected into the divided intestine, and after a certain interval, the effects of the poison were manifested in the system. Without intending to throw the least reflection upon the fidelity of the narrator, or the skill of the experimentalist, I cannot but remark, that I conceive, in so complicated an operation, it would be impossible to guard against various sources of inaccuracy,

which they possess in absorption, it appears a well-established principle, that the only use of the lacteals and the lymphatics, is to absorb certain substances that are presented to their orifices; it will now, therefore, remain for us to inquire, what is the distinctive function of each of these systems of vessels, or what is the nature of the substance which they each of them respectively absorb? With regard to the lacteals, the question is easily answered; the only substance which they are destined to receive, is the chyle; and they appear to be the only vessels which are ever employed in conveying this substance from the intestines, where it is produced, to the thoracic duct.⁵ We may,

that would essentially interfere with the inference that we must draw from the experiment.

⁵ It is not intended, by this observation, to deny absolutely that extraneous substances are never, under extraordinary circumstances, admitted into the lacteals. The earlier experimental physiologists generally agreed, that colouring substances might be detected in the chyle; this was especially the case with Lister and Musgrave's experiments on indigo; *Phil. Trans.* for 1683, No. 143. p. 6; and for 1701, No. 270. p. 819. No. 275. p. 996; and, what is more important, their results were confirmed by Haller; who informs us, that he repeated the experiments with success; *El. Phys.* xxiv. 2. 3. This is also stated as the result of J. Hunter's experiments; *Med. Comment.* p. 44. et seq.; and Cruikshank assents to the opinion; *On the Absorbents*, c. 8. But it is generally agreed that the power of these vessels in admitting the introduction of extraneous substances is very limited; and the late experiments of MM. Magendie, Flandrin, and Dupuytren, tend to show, that even this very limited power does not exist; see *Physiol.* t. ii. p. 168, 9; where it is stated, as the result of direct experiment, that when alcohol, camphor, &c. are mixed with the food, the sensible properties of these substances

therefore, consider the lacteals as the immediate agents in nutrition, by which the matter, after being duly

are detected in the blood, but never in the chyle. These experiments, I may observe, are directly opposed to those of Hunter; while, on the other hand, we are informed, that they agree with the results which had been obtained by Hallé; see Fourcroy, *Syst.* v. x. p. 91. We have also a similar kind of experiment stated in a general way, in the *Edinburgh Med. Journ.* v. xix. p. 154, 5; where a quantity of starch and indigo was confined in a portion of the intestine, when it was found, upon examination, that none of it had entered the lacteals. We have, also, a very elaborate train of experiments by the active and intelligent physiologists, Tiedemann and Gmelin, which appear to have been conducted with great attention to every circumstance that might affect their accuracy, the results of which confirm the conclusions of M. Magendie. Their object was to ascertain whether any direct communication exists between the digestive organs and the blood-vessels, except through the route of the lacteals, and the thoracic duct. The experiments consisted in mixing with the food of certain animals, various odorous, colouring, and saline substances, which might be easily detected by their sensible or chemical properties, and, in comparing, after a proper interval of time, the state of the chyle with that of the blood in the various mesenteric veins. The odorous substances employed were camphor, musk, alcohol, oil of turpentine, and assafoetida; these were generally found to be retained in the system, so as to be detected in venous blood, and in the urine, but not in the chyle. The colouring matters were sap-green, gamboge, madder, rhubarb, alkanet, and litmus; these appeared, for the most part, to be carried off without being absorbed; while the salts, viz. potash, sulphuro-prussiate of potash, muriate of barytes, muriate and sulphate of soda, acetate of lead and of mercury, and prussiate of mercury, were less uniform in their course. A considerable portion of them seemed to be rejected, while many of them were found in the urine, several in the venous blood, and a very few only in the chyle. Hence the authors conclude, that

elaborated in the digestive organs, is transmitted to the blood, for the purpose of being assimilated to this fluid, and finally employed in repairing the waste that is necessarily occasioned by the separation of the various secretions.

With respect to the lymphatics, although it would appear that they, at all times, contain a greater or less quantity of the transparent fluid, from which their name is derived, yet we have reason to suppose that their contents are of a more miscellaneous nature than those of the lacteals. If we adopt the Hunterian hypothesis, we must suppose that all the constituents of the body, as well as a variety of other substances, which are either intentionally or accidentally placed in contact with the extremities of the lymphatics, are capable of entering into them, and of being conveyed along them to the thoracic duct. And if we embrace the opinion of M. Magendie, that the function of absorption is divided between the lymphatics and the veins, or even principally carried on by the latter, there are many morbid phenomena, which seem to prove that extraneous bodies of various kinds are capable of passing along them.⁶ How far the substance

the odorous and colouring substances never pass into the lacteals, and that saline bodies do so occasionally only, or perhaps incidentally; the whole of them are, however, found in the secretions, and they must, therefore, have entered into the circulation by some other channel than the lacteals; *Edin. Med. Journ.* v. xvii. p. 455. et seq.

⁶ It is obvious that, upon the hypothesis of M. Magendie, this variation will not take place in the contents of the lymphatics,

which is conveyed by the lymphatics may occasionally serve for the purposes of nutrition, it is not, perhaps, very easy to ascertain; but we may venture to assert that nutrition is not their sole, or even their primary function. This, we can scarcely doubt, is the appropriate office of the chyle; and although it may be

or at least in a much less degree. And it must be admitted that the properties of the lymph seem to be more uniform than might have been expected, had it been composed of, or formed from, all the different constituents of the body. According to M. Magendie, it bears a strong analogy to chyle, especially in the characteristic property of separating by rest into two parts, one more solid and fibrous, and another which remains fluid, and more resembles albumen; *Physiol. t. ii. p. 171, 2.* M. Chevreul has given the following analysis of the lymph of a dog; *ibid. p. 173:*

Water	926.4
Fibrin	4.2
Albumen	61.
Muriate of Soda	6.1
Carbonate of do.	1.8
Phosphate of Lime.....	} .5
Do. of Magnesia ..	
Carbonate of Lime	
	<hr/> 1000.0 <hr/>

Mascagni, however, says, that the lymph is not uniform in all parts, but that it partakes of the properties of the contiguous substances, bile, fat, &c. *Vas. Lymph. Hist. p. 1. § 4. p. 28, 9.* A similar opinion is maintained by Blumenbach, *Inst. Physiol. § 438. p. 237.* Hoffmann's Observations on the Lymph, considering the period when they were written, are not without their value; *Med. Rat. lib. i. § 2. ch. iii.*

admitted, that under peculiar circumstances, or for a limited period, the lymph may contribute to the support of the system; yet every analogy would induce us to suppose nutrition to be no more than a temporary, or secondary office of the lymphatics.

We are indebted to J. Hunter for the first consistent hypothesis upon this subject; and I conceive that it must be regarded as one of the most important physiological doctrines which we owe to his genius. He conceived that the primary use of the lymphatics is to mould and fashion the body, so as to give it its proper form, and to enable it to increase in bulk, while its individual parts retain their appropriate figure, and proportionate size. When we reflect upon the mode in which an organized part is enlarged, we perceive that it does not grow by accretion, like a crystal, nor by simple distention, a process which is inconsistent with its texture, and the nature of its composition. We shall find, on the contrary, that it grows by an increase of each individual part of which the whole structure is composed. With respect to the muscles, for example, the number of fibres are augmented, at the same time that each individual fibre is increased in size, that the same thing takes place with respect to its tendinous extremities, and the other membranous parts that are dispersed through its body; so that if we compare the structure and composition of the corresponding muscles in their different periods of growth, we shall find the same general relation between its parts, with regard to their size and situation, at the same time that the bulk of each is increased.

The same position is, perhaps, still better illustrated by observing what occurs with respect to the bones. We observe the bone of a young animal to possess a characteristic shape, to have a certain number of projections and depressions in its different parts. If we examine the same bone in the adult animal, we shall find a general correspondence between the parts of both; we have, for the most part, the same number of projections, and bearing the same relation to each other. But it is obvious that this change of shape would not have been effected by the accretion of new matter to the original bone, nor by the distention of the parts already existing; in short, the only way in which it could have been brought about, is by the removal of the particles of which the young bone was formed, and the gradual deposition of others in their proper situations, so as to produce the adult bone. Now, an operation of this kind can have been effected by no means with which we are acquainted, except by absorption; and hence we conclude that the lymphatics alone, or at least in conjunction with the veins, are the agents employed for this purpose.⁷

The action of the absorbents is still more strikingly displayed in many morbid states of the system, where

⁷ A good view of this hypothesis, as well as of the opinions which were at that period sanctioned by the most eminent anatomists of the age, may be found in Winterbottom's *Inaug. Diss.*, published in 1781; *Thes. Med.* t. iv. p. 263, et seq.; the only reference which he gives is to Hunter's lectures. See also Cruikshank on the Absorbing Vessels, p. 108, 9. The doctrine was very admirably laid down by Mr. Allen, in his *Lectures on the Animal Economy*.

the effects are more visible to the eye than in the ordinary operations of the body in its natural and healthy state. Whenever, from any cause, an effusion of a fluid, or a deposition of a solid, occurs in an unnatural situation, we find that these extraneous substances are gradually removed. We also observe that parts, even while in their natural situation, are capable of being removed by the action of pressure; where the source of supply is, by any means, cut off from a part, even although the part itself is not otherwise affected, its particles are gradually abstracted, until it finally becomes, in a great measure, obliterated. The examples that we have of this kind are often very extraordinary, as, for example, where we find the pressure of a soft part, such as the pulsation of an artery, or of an aneurysmal tumour, sufficient to wear down the texture of the hardest bone.

The lymphatics, with or without the aid of the veins, are the only agents which can be supposed to produce these effects, yet the substances which compose them, or the fluids which they contain, cannot act as direct solvents of the bones, because they are not brought into contact, nor, if they were so, are they adapted for this kind of action. The removal of the component parts of the body, by means of the absorbents, we find to bear no relation either to the mechanical texture of the parts; nor, so far as we can judge, to their chemical composition. Thus, if a pulsating tumour be so situated as to press at the same time upon a bone and a muscle, we shall find that the earthy part of the one, and the fibrin of the other, will be removed; while the membranous part remains,

in either case, little affected. If, however, the pressure be still continued, the membranous basis begins to be absorbed; thus exhibiting the singular fact of a body, while in a soft and flexible state, wearing down a similar substance in a more dense and compact form. This property in the absorbents seems to be obviously connected with that principle in the animal economy, to which I have so frequently had occasion to refer; that the matter of which the organs of the body are composed, during the performance of their various functions, undergo some change, which renders it no longer fit for its original purpose; and that it therefore becomes necessary for it to be removed, and for new materials to be deposited in its place. We seem to have no means of forming even a plausible conjecture concerning the nature of this change, whether it be chemical or mechanical, but whatever it be, we may conceive that an important secondary purpose is served by it, viz. the one which has been described above, that of moulding the form of the body, and regulating its increase. Hence we arrive at the conclusion, that the lacteals and the lymphatics are both of them essential to the growth of the body, although in a different way; the lacteals procure the materials, and convey them into the blood, whence they are abstracted by the secretory arteries, while the lymphatics regulate the mode of their deposition, and contribute to reduce the parts into their proper form and dimensions. Upon this principle we can comprehend the mode in which an organized body may increase in size and receive the addition of new

matter, without affecting the relation between its individual parts, an object which could not possibly be accomplished by the mere addition of new particles, in whatever way they were added to the former, without these being, at the same time, removed.*

§ 3. *Mode in which the Absorbents act.*

In considering the mode in which the absorbents act, there are two distinct subjects that present themselves for our inquiry; how do the substances enter the mouths of the absorbents, and how, after they have entered, are they conveyed along the trunks? With respect to the lacteals, I have described the peculiar apparatus, which is said to be attached to their mouths, called villi, consisting of a number of small vessels, that are so disposed, as to be brought into direct contact with the substances which are intended to enter them. It has been generally supposed that the fluids enter the villi upon the principle of capillary attraction, and that the object of this peculiar structure is to obtain a number of these capillary vessels, which, in consequence of their minuteness, may be more active in the process

* Prof. Monro Tert. gives a summary view of the functions of the lymphatics in his *Elements*, v. ii. p. 598, 9, which, upon the hypothesis of the non-absorbing power of the veins, is just and comprehensive, except in regard to the share which they have in regulating the growth of the body; the same omission occurs in Blumenbach's account of Absorption; *Inst. Phys.* sect. 29. Cruikshank enters fully into the proofs of the absorption of solids, and the inquiry into the mode in which it is accomplished; *On the Absorbing System*, p. 108, et seq.

of absorption.⁹ This supposition is not, however, without its difficulties. The structure and physical properties of these villi have been thought not to be peculiarly well adapted for the purpose of this species of attraction, if we are to suppose that it operates in the same manner in them, as it does in rigid or inorganic tubes. But before we can form a decided judgment upon this point, we must determine exactly in what sense we are to employ the term capillary attraction; whether we are to refer it merely to a mechanical action, or whether we are to suppose that there is an attraction between the tube and the fluid, of a kind which may be termed elective, whether it has the power of taking up some substances in preference to others, although possessed, as far as we can perceive, of the same physical properties.

There are many circumstances connected with the lacteals, which lead us to conclude that they exercise, to a certain extent, this power of selection, and that there is a specific attraction between the vessel and the fluid, which causes the latter to enter into the former.¹ So far as we are able to judge, when par-

⁹ Boerhaave supposed that the peristaltic motion of the intestines had a considerable influence in propelling the chyle into the mouths of the lacteals; *Prælect.* § 103. t. i. p. 233, 4; but I conceive that this would be as likely to produce an opposite effect.

¹ Most of the eminent modern physiologists admit of a certain species of elective attraction, although they differ somewhat respecting its extent and its relation to the other vital powers. Bichat conceives that absorption is a vital action, in which there is a relation between the vessel and the fluid, which is of an elective

matter, without affecting the relation between its individual parts, an object which could not possibly be accomplished by the mere addition of new particles, in whatever way they were added to the former, without these being, at the same time, removed.⁸

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exists between the vessel and the particles, and that a certain vital action must, at the same time, be exercised by the vessel, connected with, or depending upon its contractile power, which may enable the particles to be received within the vessel, after they have been directed towards it. This contractile power may be presumed to consist in an alternation of contraction and relaxation, such as is supposed to belong to all vessels that are intended for the propulsion of fluids, and which the absorbents would seem to possess in an eminent degree.²

The conclusion which would follow from this view of the subject is, that an attraction exists between the mouths of the lacteals and the chyle, which seems to be analogous to, or identical with, the elective attraction which unites different chemical substances, that the lacteals, as well at their extremities as through their whole extent, are possessed of contractility, by which the fluids, when they have once entered, are propelled along them, an effect which is

² Sheldon remarks, "that they are the most irritable of any system of vessels in the human body;" *On the Absorbents*, p. 28. Dr. Young, on the contrary, seems to doubt whether the absorbent vessels possess any peristaltic motion, and is inclined to ascribe the whole effect to capillary attraction; he cites the analogy of the lachrymal duct, the contents of which he conceives are propelled entirely by capillary attraction, the duct itself being altogether passive; *Med. Lit.* p. 112. Mascagni supposes that they do not possess contractility, because he could not detect the fibres; *Vas. Lymph. Hist.* pl. 1. sect. 4. p. 27, 8. Cruikshank supports the doctrine of the irritability (contractility) of the absorbents; *On the Absorbent System*, c. 12.

probably promoted by the pressure of the neighbouring parts, while the numerous valves, with which they are furnished, prevent the retrograde motion of their contents.³

Our inquiries have been hitherto more immediately directed to the action of the lacteals; we must now examine how far we are to suppose that a similar kind of action takes place with respect to the lymphatics. There is, perhaps, in this case, an additional difficulty concerning the extremities of the vessels, and the mode in which their contents are, in the first instance, received by them, for there is reason to suppose that the transmission of the fluids themselves is conducted upon the same plan with that of the lacteals. With respect to their extremities, as we are not able to trace them to their commencement by our anatomical examinations, we can form no judgment except from analogy, and when we consider how very little is actually known respecting the mouths of the lacteals, it will appear that analogy,

³ This was in substance the doctrine of Haller; *Prim. Lin.* c. 25. § 568. He supposes that the fluids enter by capillary attraction, aided by the peristaltic motion of the intestines, and that the contents are afterwards carried forwards by the contractile force of the vessels. MM. Tiedemann and Gmelin, in the valuable dissertation to which I have referred above, maintain that the absorbents, as well as the blood vessels, possess a vital power, by which they propel their contents, but which is different from irritability (contractility). They promise to enter into the explanation of this supposed contractile power at some future period; as I am not aware that this has yet been done, I forbear to make any observations upon their hypothesis.

in this instance, can scarcely afford us any assistance. We are in fact in almost total ignorance about the whole subject; we do not know where they are situated, with what parts they are connected, how they are brought into contact with the substances which they receive, nor by what power they are enabled to take them up.

There appears likewise to be much more difficulty in ascertaining the nature of the contents of the lymphatics than of the lacteals. The lymph, so far as it has been made the subject of actual examination, is said to be nearly uniform in its nature, but the experiments that have been made upon it are not numerous, and probably did not admit of any great degree of accuracy.* And we seem to have very decisive proofs, from the inflammation or other visible effects produced, that the lymphatics are capable of absorbing a great variety of substances, differing from

* M. Magendie supposes that the lymph does not originate from the decomposition of the component parts of the body that had been previously organized, but that it consists of a certain part of the blood, which, instead of returning by the veins to the heart, is carried to this organ through the lymphatics and the thoracic duct. The great argument for this opinion is the uniformity in the properties of the lymph, and their analogy to those of the blood; *Physiol. t. ii. p. 196, 7.* Berzelius entertains a peculiar opinion respecting the origin of the lymph; he supposes that there is, in many parts of the body, a substance, which is composed of decayed animal matter, united to the lactic acid, and the lactate of soda, &c.; this is absorbed, carried to the blood, and discharged by the kidney; *View of Animal Chemistry, p. 82.*

each other the most widely in their nature, so that it would almost appear, as if, by a certain mode of application, any substance might be forced into them. Nor is this conclusion affected by the hypothesis of M. Magendie; for although we might agree with him in supposing, that, in the ordinary operations of the system, the veins are the principal, or even the sole instruments in removing the materials of which the body is composed, yet we have unequivocal evidence, that when certain poisonous or medicinal agents are applied to their extremities, they may be received or forced into them, and conveyed into the circulation. The case of the metallic or other medicinal substances, that are taken up by the lymphatics, may appear to be less difficult to explain, because the absorption is generally produced by friction, or some mechanical process, which may be supposed to force the substance into the mouths of the vessels, or to produce an erosion of the epidermis, which may enable the substances to come into more immediate contact with the mouths of the vessels. We may also imagine, that when the component parts of the body are brought into close approximation with their capillary extremities, they are then taken up in the same way that the chyle is absorbed from the intestines.

There is moreover a peculiar difficulty which attends this part of our subject, arising from the circumstance, that the densest solids are absorbed as well as the more fluid components of the body. What are we to conceive of the intimate nature of

this operation? If solution of the substance be necessary, we are at a loss to find a proper solvent; many of the substances are insoluble in water, or in the serous fluid which is found in the vessels, while, on the other hand, it is, perhaps, not easy to conceive, how the substances can be absorbed, without being previously dissolved, and still more so, how the solids can have their texture broken down, and enter the vessels, particle by particle, as it were, and be suspended in the lymph in a state of extreme comminution.⁵

Physiological writers, when treating upon this subject, have been frequently in the habit of employing expressions, which, we may presume, they must have intended to be taken in a metaphorical sense only, as when they speak of the solids being corroded by the lymphatics or broken down by them. But such a phraseology, it is evident, can amount to nothing more than an expression of the fact in different terms, and certainly affords us no insight into the mode in which it is effected.

This is one of those questions in which the metaphysical physiologists have brought forwards the operation of the vital principle, for the purpose of ex-

⁵ Monro Sec. in his *Treatise on the Brain*, c. 5. enumerates the arguments which have been generally adduced to prove the absorption of the solids. The most direct and unexceptionable of them are various morbid occurrences, in which tumours, or certain portions of the components of the body are removed; the facts that are brought forwards are by no means of equal value, but, upon the whole, they may be regarded as amounting to a proof of the position.

plaining the difficulty.⁶ It is said that as long as a part possesses the vital principle, this agent enables it to resist the action of the absorbents, but that it immediately becomes subject to their influence when it loses this principle. We have, indeed, abundant evidence to show, that dead matter is more easily acted upon by the absorbents than the same matter while it retained its vitality; indeed we may go farther, and safely affirm, that no part can be absorbed until its texture is destroyed, and consequently until it is deprived of life. No substance can possibly enter the absorbents while it retains its aggregation, so that it necessarily follows, that the preliminary step to the absorption of a body is its decomposition. We are then to inquire how this is effected, and what connexion the means so employed have with the subsequent absorption of the decomposed matter.

We may conclude that the first step in this series of operations is the death of the part, by which expression is meant, in the present instance, that it is no longer under the influence of arterial action. It therefore ceases to receive the supply of matter which is essential to the support of all vital parts, and the process of decomposition necessarily commences. It would appear, however, to be a principle in the animal œconomy, that an organ remains under the influence of the absorbent system, after it ceases to be connected

⁶ Blumenbach, on this occasion, resolves the difficulty by the mysterious operation of his *vita propria*; *Inst. Physiol.* § 436. I have not been able to peruse the treatises of Albrecht, and of Ontyd, to which he refers.

with the arterial. It therefore becomes subject to those causes which tend to its dissolution; but, as we may presume, before the operation is fully established, or while it occurs in each successive portion of which the whole is composed, the portion so changed is taken up by the lymphatics. There are various circumstances which may be conceived to be efficient in cutting off the ordinary supply by the arteries: a deficiency of nutritive matter in the system at large, a local disease or derangement of the arteries that belong to the organs, the obliteration of the arteries by pressure or any kind of mechanical obstruction, may be enumerated among the probable or possible causes which might produce the death of the part, at the same time that the absorbents connected with it may retain their activity.⁷

And in many instances, without the intervention of any thing that can be properly stiled morbid, the same kind of effect will be produced, although in a less degree. According to the principle of perpetual change, which pervades the whole organised system, we have the two operations, of accumulation and of expenditure, always going forwards. In the state of the most perfect health and vigour, and when the body has attained its complete size and form, these operations should exactly balance each other; but any deviation from this exact balance, by which the relative action of the capillary arteries and the absorbents is affected, will destroy this equilibrium.

⁷ See the observations of Mr. Bell; *Anat. v. iv. p. 311, &c.*

The part of this process, which is the most difficult to comprehend, is the nature of the change, which the materials of the body undergo, when they cease to be under the influence of the arteries, and are converted into that state which adapts them for being taken up by the absorbents, whether the change be mechanical, consisting merely in the comminution of the substance, or chemical; and if the latter, what is the exact nature of the change, and how is it effected. These are points upon which I conceive that we are totally ignorant, so that I shall offer no conjecture upon the subject.

I have given an account above of the recent experiments of M. Magendie on the organs by which absorption is performed, and we are also indebted to the same physiologist for a new hypothesis respecting the mode in which the organs act. Having ascertained, by a previous train of experiments, the degree of effect which certain narcotic substances produce upon the system, so as to be able to refer to these as a standard of comparison, he was induced to examine how far the absorbing power of the vessels was promoted or retarded by the states of plethora or of depletion. The result seemed to be, that plethora uniformly retarded, and depletion as constantly promoted absorption, and hence he concludes, that it must consist in a mere mechanical action, independent of any principle connected with vitality. Proceeding upon this position, which, however, does not appear to be, in any respect, a necessary consequence of the premises, he conceives that the only physical action which can satisfactorily explain the phenomena, is that of

capillary action exercised by the sides of the vessels upon the substances to which they are exposed.⁸

So far M. Magendie's hypothesis may seem to agree with the one which is generally adopted, but upon farther investigation, it will be found to do so rather verbally than really, for we find that he does not suppose that the absorbed fluid enters by the open mouths of the vessels, but that it is imbibed by the substance of the vessel itself, or rather filters through its parietes, and that, when it has entered by this means, it is then carried forwards by the current of the fluid previously contained in the vessel. To prove the possibility of the hypothesis, he performed experiments on the veins shortly after death, and found that they were capable of imbibing and transmitting a sensible quantity of a fluid that was presented to them, and that the same also took place, although less readily, with respect to the arteries. To complete the proof of the hypothesis, M. Magendie endeavoured to execute a series of analogous experiments upon the vessels of a living animal. They consisted in taking a portion of a vein, the jugular, for example, in completely detaching it from its connexions of every kind, and dropping upon its external surface a solution of a deleterious substance, the effects of which were quickly manifested in all parts of the system.⁹ But I may remark on this, as I have done

⁸ "Parmi les conjectures que l'on pouvait se permettre à cet regard, celle qui ferait dépendre l'absorption de l'attraction capillaire des parois vasculaires, pour les matières absorbées, était sans doute la plus probable." *Journ. Physiol.* t. i. p. 6.

⁹ *Journ. Physiol.* t. i. p. 9, 0.

on a former occasion, that without impeaching either the veracity or the address of the experimentalist, the operation appears to be of that complicated nature, which it would be impossible to perform so as to preclude all sources of inaccuracy, and those such as would materially interfere with the result.

With respect to the absorption of solids, it would follow from this view of the subject, and, indeed, it is admitted by M. Magendie to be the case, that nothing can enter the vessels by this kind of filtration, except what is perfectly dissolved in the fluids; he does not, however, inform us, in what manner the fluids which are contained in the vessels, or rather those that are contiguous to them, can effect this solution. Upon considering the doctrine of M. Magendie in all its parts, I cannot but regard it as both antecedently improbable and unsupported by facts or analogies; that it is very difficult to conceive how the various substances which are absorbed, whether by the lymphatics, or by the veins, can enter by this kind of filtration or transudation, while it affords no satisfactory explanation of the cause why some substances are taken up in preference to others, or of the mode in which the absorption of the solids can be accomplished.¹

¹ The doctrine of the transudation of fluids through the vessels during life, was maintained by many of the mechanical physiologists of the last century; it entered largely into the speculations of Kaau Boerhaave; see his treatise, *de Perspiratione*, c. 27. *passim*; and is admitted by Haller, *El. Phys.* ii. 2. 23. Wm. Hunter very decidedly supported the same opinion,

The conclusions which were formed by M. Magendie upon this subject, have been more lately enforced by a series of elaborate and ingenious experiments, that have been executed by M. Fodera. He commences by stating a number of facts, which tend to prove that the membranes generally, and the coats of vessels in particular, permit fluids to filter through them, and that this may take place before their texture or organization is visibly changed by the process of decomposition. But a more important and interesting part of his investigations refers to the power which the vessels of the living body possess, of allowing fluids to enter them by filtration; or, as he terms it, by imbibition.

In order to prove this point, he injected into two separate cavities of the body two fluids; which by their union produce a compound, the presence of which may be obviously and unequivocally indicated; and which could only be formed by the two ingredients coming into contact with each other. The cavities of the pleura and the peritoneum were selected for this purpose; into one the ferro-prussiate of potash, and into the other the sulphate of iron were injected, and the result was, that upon examining the

Medical Comment. c. 5; and an opinion very similar to it is adopted by Mascagni, pt. 1. sect. 1. p. 6, 7. Cruikshank, on the contrary, endeavours to refute the hypothesis, and observes, that many of the appearances which have been observed upon dissection, and which have been thought to prove transudation during life, were in reality the effect of what occurred after death; On the Absorbent System, p. 11, 14.

body after a certain interval, many of the membranous and glandular parts, connected with the abdomen and thorax, were tinged with a blue colour. If the animal was left untouched, these phenomena took place slowly; but the operation was very considerably promoted by the application of galvanism. This effect was exhibited in an experiment analogous to one of M. Magendie's, in which a solution of ferro-prussiate of potash was enclosed in a portion of intestine, while a cloth, soaked in sulphate of iron, was placed on its external surface; when, upon the transmission of the galvanic influence, the cloth was instantly stained of a blue colour. It was farther observed, that according to the direction of the current, the blue colour might be produced either on the outside or the inside of the intestine.

From a number of experiments of this nature, M. Fodera comes to the following conclusions: 1. That exhalation and absorption may be referred to transudation and imbibition, through the pores of the membranous textures, "*capillarité des tissus*," which enter into the composition of the organs. 2. That this double effect may be produced in all parts of the body, and that the fluids which are imbibed may be conveyed equally by the lymphatics, or by the arteries and veins.²

It must, I conceive, be admitted that these experiments go very far to prove that membranes, perhaps even during life, and certainly after death, before their

² Magendie, Journ. t. iii. p. 35, et seq. This paper consists of an abstract of M. Fodera's Mem. by M. Andral, jun.; the original is, I believe, still unpublished.

texture is visibly altered, have the power of permitting the transudation of certain fluids; but before we can assent to the doctrine of M. Fodera, that absorption generally is effected in this manner, and that the chyle enters the lacteals simply by filtration, or imbibition through the coats of the vessels, there are many preliminary difficulties to be cleared away, and many additional circumstances to be ascertained, in order to complete the analogy. Although there may be a strong resemblance, or even a perfect identity between the chemical composition of a membrane and a vessel, their physiological texture, which, in the present case, is more particularly concerned, is probably very different. And, if the fluids can enter the vessels by this species of filtration, what reason can be given why they should not have an equal tendency to transude again out of the vessel? What cause can be assigned why they should enter the vessels at all, when there would appear so much easier a passage from one part of the cellular substance to some contiguous portion of the same texture?

Besides, when we reflect upon the nature of the experiment, in connexion with the inferences that are deduced from it, we find that it implies a degree of correctness in the execution, and of attention to a variety of concurrent circumstances, greater, perhaps, than ought to be ascribed to any physiologist, of whatever skill or dexterity he may be possessed. It requires that there should be not a single divided extremity of an artery, or of a vein, nor a single open cell of the membranous texture exposed to the fluid employed; for if any portion of it should

enter into either of these organs, the essence of the experiment is destroyed. And, even granting that all these difficulties could be counteracted, still it would by no means follow, that because an active chemical solution can pass through a membrane, or the coat of a vessel, that the same could be done by the chyle. I feel, therefore, no hesitation in asserting that, curious and interesting as are the experiments of M. Fodera, they do not prove the position which they profess to establish; and that they ought not to affect our ideas respecting the doctrine of absorption.³

We are indebted to Dr. Barry, whose observations on respiration I have already had occasion to refer to,⁴

³ It is but justice to M. Fodera, to remark, that the full detail of his experiments, so far as I am able to learn, has not yet been given to the public. I think, however, that it is not unfair to conclude that the report of them, which is inserted in M. Magendie's journal, may be regarded as exhibiting a correct account of them; and that no circumstance would be omitted, which could be fairly urged in their favour.

⁴ Since I wrote the short appendix to the 7th chapter, I have had the pleasure of perusing the detailed account of Dr. Barry's experiments, together with the report of them made to the Academy of Sciences, by MM. Cuvier and Duméril;* and I had likewise the good fortune to be present at some experiments which were performed by Dr. Barry himself, at the Veterinary College. Making those allowances for unforeseen and unavoidable difficulties, which it is but fair to admit on such occasions, I should say that it appeared sufficiently obvious, that when one end of a glass tube was inserted either into the large

* *Recherches experimentales sur les causes du mouvement du sang, &c.*

for a series of experiments which he performed in order to prove, that absorption depends altogether upon atmospherical pressure. The experiments, which appear to have been sufficiently numerous, and to have been attended with very decisive results, consisted in introducing into a wound a portion of some poison, the effects of which had been previously ascertained, and to compare these with what took place when the pressure of the atmosphere was removed, by the application of an exhausted cupping-glass over the wound. The results appear to have been very remarkable; and, in a practical point of view, cannot fail to prove of great and obvious utility. The same dose of poison, which,

veins, into the cavity of the thorax, or into the pericardium, the other end being plunged into a vessel of coloured water, the water was seen to rise up the tube during inspiration, and to descend during expiration. We may hence conclude, that under the circumstances in which the experiment was performed, there was less resistance to the entrance of the venous blood into the heart during inspiration than during expiration; but it will still remain for us to inquire whether the principle will apply to the organs in their natural and entire state, or in what degree it is applicable. Now, there are some facts, which would lead us to suppose, either that the effect does not take place, or that it does so in a very slight degree only. 1. During each act of respiration, we have three or four pulsations of the heart, which are exactly of the same strength, although they must have occurred during the various states of the thorax. 2. In the fœtus, where the lungs are quiescent, we have still the circulation proceeding without any apparent difficulty. 3. There are various tribes of animals that possess a circulating system; but which are either without lungs, or have them constructed upon a principle entirely different from those of the mammalia.

under ordinary circumstances, destroyed an animal in a few seconds, was rendered completely harmless by the operation of the vacuum ; and when the symptoms had commenced, and even when they had proceeded so far as to impress the spectators with the idea that the life of the animal was destroyed, still the vacuum had the effect of speedily and entirely removing them ⁵.

Important, however, as these experiments are in a practical point of view, there appear to me to be certain circumstances, which must be taken into account before we can admit of the hypothesis that is deduced from them. In the first place, it would seem that the poison was not simply laid upon the surface, but was inserted into a wound ; hence it would be immediately mixed with the blood, and be carried by the veins to the central parts of the system. Now, it is obvious, that when a vacuum is formed over the divided end of a vessel, and especially of a vessel which is supposed to be passive, or to be influenced only by physical causes, the motion of the fluid through this vessel

⁵ Dr. Barry presented an account of his experiments on absorption, and of the inferences which he deduced from them, to the Med. Chir. Society ; it is from this source that I derive my information. The following are two of Dr. Barry's positions : "That the whole function of external absorption is a physical effect of atmospheric pressure." "That the circulation in the absorbing vessels, and in the great veins, depends upon this same cause, in all animals possessing the power of contracting and dilating a cavity around that point, to which the centripetal current of their circulation is directed." Dr. Barry explicitly states his opinion, that vital action is not concerned in absorption.

must be retarded. And this would be equally the case, upon whatever principle we supposed the fluid to be propelled, or carried through the vessel in question.

But it does not necessarily follow that this would be the case in the natural state of the parts, when the vessels remain entire, and the atmospheric pressure is exercised equally upon all the contiguous organs. Nor does it appear to be an obvious consequence of Dr. Barry's experiments, that the same effect would ensue, if we had it in our power to apply a poisonous substance to the extremity of a lacteal or a lymphatic, provided as these vessels are with valves, and exercising a contractile power over their contents. The immediate effect would be the distention of the contiguous parts, and the dilatation of the vessel itself; but, provided the extremity of the vessel remained closed, this operation might promote, rather than retard, the progress of its contents.

In the last place, I conceive that it is altogether impossible to apply Dr. Barry's principle to the action of the lacteals; they appear to be so far removed from the influence of atmospheric pressure, that we must suppose their contents to be propelled by some inherent power in the vessels themselves, or by some mechanism immediately connected with them; and presuming this to be the case, we have a very strong analogical argument for supposing, that the function of absorption, in the other parts of the system, is conducted upon the same principle.

When we examine the extent of the lymphatic

system, and endeavour to trace out its connexion with the various parts of the body, we observe that a great number of these vessels have their origin from the neighbourhood of the cutis,⁶ and it has therefore been supposed that absorption is carried on to a considerable extent by all parts of the surface. The doctrine of cutaneous absorption seemed to explain a great variety of phenomena in the animal œconomy, both physical and pathological, and was generally had recourse to as one of those operations, which had a powerful influence upon the functions of the body, both in their natural and their morbid condition. That under certain circumstances the absorbents are able to take up substances applied to the skin, especially when aided by friction, is sufficiently proved by the effect of various medical agents which are enabled by this means to enter into the circulation, and to act upon the system in the same manner as if they had been received into the stomach. Thus mercury applied to the surface produces its specific effect upon the salivary glands, and lead upon the muscular fibre, while opium, tobacco, and other narcotics, manifest their peculiar action upon the nervous system.

But besides this absorption of substances applied to the skin, and forced into the mouths of the vessels by friction or other mechanical means, it was an opinion very generally embraced by physiologists,

⁶ We have a view of the cutaneous lymphatics, as far as they can be rendered visible by injections, in Haase, *de Vas. Cut. et Intest. Absorb.* tab. fig. 2; also in Mascagni, tab. 2. fig. 9.. 28. and tab. 3.

that when the body is simply immersed in water, the cuticle still remaining entire, the same kind of absorption takes place, and even that the skin has the power of imbibing water from the atmosphere, when it exists there in any unusual quantity. This was the opinion of Sanctorius, and of all those who, since his time, performed experiments of a similar kind; the weight which the body was found to have gained, under certain circumstances, when this could not be explained by the excess of the sensible ingesta above the egesta, was attributed to cutaneous absorption. We are now, indeed, aware, that a part at least of what Sanctorius, and the statical experimentalists ascribed to cutaneous absorption, depends upon the action of the lungs, but still, after this deduction, it was conceived that a certain proportion of this excess of weight could only be accounted for upon the supposition of the absorption by the skin.⁷

This doctrine was, however, some years ago, called in question by Seguin, who attempted to disprove it by a series of direct experiments, which were certainly ingenious, and would be almost decisive against it, were they not subject to certain causes of inaccuracy which it is not easy to obviate. The experiments consisted in immersing a part of the body in the solution of a saline substance that acts in a specific

⁷ Mascagni, in p. 22, 3. of his great work, brings forwards the various facts and arguments that have been adduced in favour of the doctrine of cutaneous absorption. See a popular view of the subject by Dr. Wilkinson; *Med. Museum*, v. ii. p. 117; also the article "Integuments," in Rees's *Cyclopædia*.

manner upon the system, which it is easy to recognize, as, for example, corrosive sublimate. Now, we are informed, that provided the cuticle be entire, we have no evidence of any of the mercury being received into the system, or of any part of the salt being abstracted from the water.⁸ Experiments tending to the same conclusion with those of Seguin's, although not so decisive in their nature and results, were performed by other physiologists. Currie carefully examined the weight of the body before and after immersion in the warm bath, but could not find that it gained any addition, and was hence led to conclude, that absorption never takes place by the skin, except when the substances applied to it are forced into the mouths of the absorbents by mechanical violence, or when an abrasion of the cuticle, or a destruction of some of the subjacent parts has taken place.⁹ This opinion seems to have been generally acquiesced in by the modern physiologists, so that it has been assumed as a general fact, that except under the particular circumstances referred to above, the cutaneous absorbents were not capable of taking up substances that are merely applied to the external surface.¹

⁸ Fourcroy, *Med. Eclair.* t. iii. p. 232. . 241. and *Ann. Chim.* t. xc. p. 185. et seq.

⁹ *Med. Reports*, ch. xix.

¹ M. Magendie, in conformity with the doctrine which he maintains respecting absorption generally, supposes that where we have evidence of this operation having been carried on at the surface of the body, the veins and not the lymphatics are the principal agents; *Physiol.* t. ii. p. 189. . 6. His observations on

This subject has been lately investigated by Dr. Edwards, with that skill and address which he has manifested on so many other points connected with the animal œconomy. The conclusion to which he leads us is, that the function of absorption is actually carried on by the skin to a considerable extent, and probably without interruption, although in different degrees, according to the condition of the animal, and the circumstances to which the body is exposed. He considers, under separate heads, the absorption which is effected by the skin, when the body is immersed in water, and when it is immersed in air. With respect to the case of absorption, while the body is immersed in water, the first experiments were performed on cold-blooded animals, and he appears to have proved unequivocally, that in them absorption takes place from the skin with great facility, and in considerable quantity. If a lizard be exposed for

this point are ingenious and deserving of attention, but it appears to me to be the least tenable part of his hypothesis. A series of experiments were performed some years ago by Dr. Rousseau, of Pennsylvania, the object of which is to prove that cutaneous absorption has no existence, even under any circumstances, and that in all those cases where substances applied to the surface are supposed to be absorbed by the vessels of the skin, the effect is in reality to be referred to the substance being converted into vapour and inhaled by the lungs; he even pushes his hypothesis so far as to suppose, that when mercurial frictions are applied to the surface, the heat of the body volatilizes the metal, and in this way enables it to enter into the system by pulmonary inhalation; see an account of the experiments by Dr. Stock, in *Ed. Med. Journ.* v. ii. p. 10. et seq.

some time to a dry atmosphere, a great proportion of its fluids are removed by transudation, and if we then immerse a part only of its body in water, we may observe a visible and copious augmentation in the quantity of its fluids, both from the appearance which it presents to the eye, and, more decidedly, from the increase of its weight. He therefore infers that a similar operation must be carried on by the human skin, that both transudation and absorption are always going forwards, and that the body gains or loses in weight, according to the excess of the one above the other. Dr. Edwards remarks at some length upon the experiments of Seguin; he does not question their accuracy, but he conceives that we are not warranted in concluding from them that no absorption takes place, because none was observed in these particular cases. He points out a variety of circumstances which may tend to diminish the absorption or to increase the transudation, so that the balance of effect shall be in favour of the latter operation, and thus, so far as the weight of the body is concerned, might seem to exclude the idea of absorption. He supposes that Seguin's experiments were performed under such circumstances, and that therefore the weight of the body was not increased, or was even diminished after immersion in the bath, an effect which would depend partly upon the absorption being retarded by a turgid state of the vessels, while the warmth of the medium would tend to promote the transudation.²

² *De l'Influence*, &c. ch. xii. p. 345. et seq.

With respect to the absorption by the surface, when the body is immersed in air, it is admitted to be less easily detected under these circumstances than in the former case; but Dr. Edwards conceives that his experiments, especially a series which he performed on guinea pigs, warrant the opinion, that absorption does take place, although in considerably less quantity than during immersion in water; and he finally concludes, that when the body loses weight during immersion, in damp air, the amount is to be regarded as the difference between the loss by transudation, and the increase in consequence of the absorption of aqueous vapour.³

§ 4. *Connexion between Absorption and the other Functions.*

Having now considered in succession the functions of digestion and of absorption, by which the aliment is first reduced to the state which is adapted to nutrition, and is afterwards carried into the circulating system, it remains for me to offer a few observations upon the changes which it undergoes from the time that it enters the absorbents, until it is finally deposited in the different organs of which it is destined to form a constituent part. We have seen that the chyle, when it is received into the lacteals, both

³ De l'Influence, &c. c. xiii. p. 556. et seq. We have an essay by Dr. Kellie, "On the Functions of the Skin," which contains a good general view of the subject up to the period when he wrote, 1805; Ed. Med. Journ. v. i. p. 170. et seq.

in its chemical and its physical properties, exhibits a considerable resemblance to the blood, the question then for us to determine is, whether, after it is poured into the veins in the imperfect state, in which the process of sanguification appears to be only in part accomplished, it is, in the first instance, mixed with, or diffused through the whole mass of blood, and that all the remaining changes are effected by the ordinary operations of secretion and excretion, or whether there be any specific process for the more complete assimilation of the chyle, by which it may be, in the first instance, converted into one or more of the constituents of the blood.

That the function of respiration, in some measure, contributes to assimilation is at least a probable opinion, and we may also suppose, that the consequence of the transudation which is carried on from all the surfaces of the body may have its effect in discharging any redundant quantity of water; but, except these, we are not acquainted with any processes which can affect the constitution of the blood, while it remains in the larger vessels. We know, however, that it is principally in these vessels that it acquires its characteristic properties, more especially that the fibrin obtains its peculiar texture and its power of spontaneous coagulation, which existed in a less perfect degree at least in the chyle, and this is still more remarkably the case with the colouring matter. It appears to be pretty well established, that the substance which gives the red colour to the blood is the vesicle that surrounds the globule; it

may also be laid down as a point which is generally admitted by physiologists, that although this substance contains iron, yet that the iron is not the immediate cause of the red colour. We have likewise reason to conjecture that it is upon this colouring matter that the oxygen of the atmosphere more particularly acts, when the blood is transmitted through the lungs, and is converted from the venous to the arterial state; yet this seems to afford us no insight into the mode in which the red colour is, in the first instance, produced, why it exists in a slight degree in the chyle, and why it is found so much more copiously in the blood. We may conclude, that it is not the consequence of mere concentration, nor are we able to refer it to any of those operations which are known to be going forwards in the animal œconomy, with the nature of which we are so well acquainted, as to allow us to speculate upon their ultimate effects.

When we consider the nature of the relation which exists between the sanguiferous and the absorbent systems, we appear to be warranted in supposing that the fluid which is contained in the arteries should be regarded as constituting the blood in its most perfect state; that it is then carried by the capillary vessels into all parts of the body, where it loses a certain quantity of its constituents, and is thus reduced to the state of venous blood, that the thoracic duct pours into the great veins the materials necessary to supply the loss which it has experienced, and that some change, either chemical or mechanical, is effected in the lungs, which again converts it into the state

of arterial blood. According to this view of the subject, the process of the conversion of venous into arterial blood, will consist in the addition of a quantity of carbon, hydrogen, and oxygen, with an undue proportion of nitrogen; in passing through the lungs a portion of carbon, oxygen, and hydrogen, are separated, under the form of carbonic acid and water,⁴ while in the course of the circulation, we may presume, that the excess of nitrogen which must be thus produced, will be separated from the blood, in the form of muscular fibre or of membrane, or still more, of urea, of which nitrogen composes so large a proportion.

It only remains to inquire into the relation which subsists between the absorbent and the nervous systems. And this, we have every reason to suppose, is of that indirect nature only, which I have had occasion to describe when treating of the action of the heart, where the function of the part is not necessarily dependent upon the power of the nerves, although there may be many cases in which it is materially influenced by this power. It appears that the examinations of anatomists indicate that there are but few nerves sent to the absorbent system, and that even these few seem rather to pass by them, in order to be transmitted to more distant organs, than to be ultimately destined for the lymphatic vessels or glands

⁴ It is scarcely necessary to remark, that the water which is exhaled from the lungs is not supposed to be generated in this organ, but merely abstracted from the pulmonary blood by a species of secretion.

themselves. The mode of action of the absorbents, (at least if we except that of their mouths, which is altogether involved in obscurity) is of that kind which may be explained without the aid of nervous sensibility, and, indeed, every circumstance connected with them, seems to show, that, like the sanguiferous system, their ordinary operations are to be referred to contractility alone.

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